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EXTRACTS FROM THE AUTOBIOGRAPHY OF A PARANOIAC.[1]

Edited, with a Commentary, by

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As a preface to the autobiographical sketch of this interesting religious paranoiac, I make a few transcripts from the medical records of the Hudson River State Hospital for the Insane, of which institution he was an inmate for over seven years, from July 29, 1872, to October 11, 1879. He was thirty years of age at the time of admission, single, and a farm laborer by occupation. He was not a church member, had a common school education, and was a native of the United States. Hereditary predisposition was not acknowledged. His mother, who accompanied him to the hospital, stated that he had always been delicate in his physical constitution and given to despondency. Since

[1] Read before the Neurological Section of the New York Academy of Medicine, Dec. 14, 1888.

the age of twenty he had done little or nothing, because of ill health. A year previous to his commitment as a lunatic to the hospital, he shot himself in the forehead, in an ineffectual attempt at suicide. Later he developed delusions that the people of the village were acting upon him by magnetism, spoke disparagingly of him, and were conspirators against his peace. During the whole of his sojourn in the hospital he had hallucinations of hearing, and in the earlier period of his stay, delusions of persecution. Toward the end of his seven years of hospital life he gradually developed, in addition, delusions of grandeur. Although he had occasional lapses of self-control, manifested rarely by the breaking of window glass or the tearing of clothing, he was for the greater portion of the time sufficiently self-possessed to restrain whatever violent or destructive inclinations he may have had, and was permitted to go out alone upon the large grounds of the asylum whenever he wished and to wander about the woods at will.

It was during the years 1878 and 1879, the last two of his stay at Poughkeepsie, while still the victim of constant auditory hallucinations and of mingled delusions of persecution, unseen agency and grandeur, that he wrote an autobiographic volume of four hundred manuscript pages, with the extraordinary title-page presented below :[1]

[1] It was first written in phonetic cipher, on pieces of brown wrapping paper, but subsequently copied in a neat hand on note paper and bound by himself in a 400-page volume. A New York publisher once had the volume in press, but the forms were destroyed and the publication suppressed by relatives of the patient.

THE

PILING OF TOPHET

AND THE

TRESPASS-OFFERING:

A

TRUE LIFE HISTORY.

BY

F. T. J——,

Patient in the
Hudson River State Hospital for the Insane,
Poughkeepsie, N. Y.

"For Tophet is ordained of old; yea, for the king it is prepared: he hath made it deep and large; the pile thereof is fire and much wood; the breath of the Lord, like a stream of brimstone, doth kindle it."—Isaiah xxx. 33.

The following is the dedicatory page:

To
Sinners,
who in every sin against their neighbor, by word or manner or responsive feeling, have, from the foundation of the world, been laying their own share of fuel on this unlighted pile;
and to
Saints,
whose acts towards their God and towards the world have been but an instinctive prelude to this supreme sacrifice,
in the name of
Nature
and its unfailing laws,
and of its
Founder
and His Eternal Kingdom,
this book
is
worshipfully
dedicated.

The book itself is a deeper history of his life and mental evolution than any but himself could furnish. It is remarkable for its excellent literary style, and for its keen reasoning and psychological analysis of his own disordered mind. In it he dissects his hallucinations and delusions like a skilled anatomist. It is as fascinating as a novel. I regret that I can give but extracts here and there which have an especial bearing upon the elucidation of his psychosis.

In the Preface he defines the scope of the book as follows:

"This work is given to the public as a lunatic's defense of his position. Every effort I have made hitherto to come to an understanding with my fellow-men, on things which I see to proceed from them, and which give my life its whole shape, has drawn out nothing more than blank denials of all knowledge of the things I spoke of. Now it is impossible for me to reduce my thought to the bounds which others have been willing to concede. The object of this little autobiography is to show the form and consistency of the thought that is in my mind.

"I present my evidence to the tribunals of last resort, the public and the press, and ask them to try the case and render their verdict. Have I a right to my thought or have I not? If not, where am I deceived? If I have, why is not mine the *true* thought for all men?"

A paragraph from the Introduction further reveals the object of his confessions:

"A person is supposed to have a reason for what he does, and I might consider it incumbent upon me to tell the motives which actuate me in thus entering upon the work of the scribe under circumstances so peculiar. Is there anything I have to tell that might not as well and more safely be left untold? It is a question which I do not have to consider and decide to-day, for I have been long inspired with the conviction, the consciousness, that I *have* something to tell that it would be worth the world's while to hear."

In another introductory paragraph he makes an excellent diagnosis of his mental infirmity. Addressing his reader he says:

"I did not tell you that I am a patient in an asylum. I am to take it for granted at the outset, that my prospective reader knows nothing of my character, condition, or circumstances, beyond what I tell him. I am here as an insane patient. I have been here over five years. . . . . Being an insane man, it will be nothing unexpected that I should, in giving these reports of my fortunes, narrate incidents and particulars partaking more or less of the marvelous or preternatural. I am not only a lunatic, but one of the class of lunatics having a controversy with the world in general; in other words, possessed with a monomania, or crazy one-sidedly, or on a single subject."

In the hospital record presented above, nothing is adduced as to heredity in his case, and but little stated concerning his mental condition in early youth. These deficiencies are, to a great extent, supplied in the autobiography. I shall permit our author first to describe his appearance in this world, in a cyanotic condition, and the characteristics of his childhood and early youth, and subsequently the hereditary influence in his destiny:

"It is said that I was entirely black when I was ushered into the world, and that for I forget how long a period of time I did nothing but give vent to heart-saddening wails. Was I lamenting the gift of light, on this morning of what was to become a woe-burdened existence?

"I was a weakly infant. I came near dying of the whooping cough, and it was always asserted by those who knew, that I owed my life to the untiring exertions of a poor woman who lived a neighbor, who busied herself all night with me, dipping me at intervals into a tub of warm water. My half-sister had it at the same time and died.

"It will be of use to give an idea of my nature and disposition in my tender years. I was always a shy, retiring child, not disposed to make myself free with strangers, not much given to prattle—in fact, one of the sad and silent sort from the first. I can remember some peculiar sensations which used to weigh on my mind, which go to show that the foundation of my mind-life was but imperfect from the first. I used to be troubled with very strange feelings when I was waking out of sleep, especially if I had been taking a nap in the daytime. It used to seem to me that I was floating in the air, and I often thought to myself: 'Why, how queer I have been feeling!' It was as if I filled the whole room, way up to the ceiling. I was told by others that I sometimes raised myself up in bed after getting to sleep and made an outcry, 'O don't, O don't!' seeming to be in great distress; but the strange part of it is that I could remember nothing about it. I do not think that I ever remembered even their waking me, or finding them at my bedside. I only had their word for it next day.

"As far as I can go back, I remember having at times, but not frequently, impressions which must be identical with what I have lately heard others speak of as 'double memory.' The feeling would all at once creep over me that the very thing I was present with, my ideas and perceptions at that time, had happened to me once before in just the same sequence and arrangement. I have heard this explained as due to a lack of simultaneity in the action of the two lobes of the brain, the tardy one remembering what had already passed through the other. My own theory was different, leaving the organ acting out of consideration. I only went so far as to look at it as a mistaken quality in the perception—an erroneous attaching of the nature of the act of remembering to what was really the act of thinking in the present.

"I was very early in life an observer of my own mental peculiarities, to a degree which I think must be a very rare exception. I often used to be sensible of an unsatisfactoriness in my consciousness of what surrounded me. I used to ask myself, 'Why is it that while I see and hear and feel everything perfectly, it nevertheless does not seem real to me? It is as if I

were in danger of forgetting myself and the place where I am!' I often wondered even how I kept the run of things as well as I did. I always found myself holding on to the orderly and proper connection of my acts, and yet from my feelings I could not have answered for my doing so. I can remember sitting at my desk in school, when a small boy, and dwelling with melancholy on this dimness in my perception of existence, and wondering how it was with others in this respect. I wondered to myself if life, as ordinarily bestowed, included this deficiency.

"I showed in my tastes and behavior a harmony with the internal composition of my mind. I was never given to the active sports which the common run of boys take so much delight in.

"The simple fact is that I had a languid nervous development, and from the necessity of my organization could not have much capacity or relish for sports of agility.

"If I could compound a boy of my own I should try to improve on the model I remember to have exhibited in myself.

"It is not true that I was regarded or treated as strange or deficient in my wits. Such an idea would look misplaced to those who knew me and consorted with me in those days. These differences are perhaps more evident to myself than they ever were to the greater part of my acquaintances. I brooded on this side of my character at a later period, and I no doubt remain liable to give greater prominence to disparaging traits than some impartial observers would justify me in doing.

"As a general rule, my harmless and peaceable disposition kept me out of squabbles with my schoolmates. If I was approached in an aggressive way I met it with absolute non-resistance, which in my case had the disarming effect which is attributed to it by pious moralists."

"If we change the scene from the playground to the schoolroom we shall find that I attained a distinction of my own apart from the average, and more to my advantage there. I was always a favorite with my teachers. I never gave them any trouble, and took to my studies with a willing relish that could not but be

pleasing to them. I learned to read before I went to school; in fact, like an old asylum acquaintance, Mr. M., inventor and infidel monomaniac, I can almost say that I can't remember when I could not read.

"I was frequently singled out for complimentary remarks on my proficiency in my studies. I gave evidence of some talents of a higher kind—could draw, for instance, better than any boy in the school.

"One of the most marked weaknesses of my character, as a child, was my susceptibility to being teased.

"After having pondered some on the traits of the human animal in this particular, I have come to the conclusion that there is no further explanation needed than that the impression made on the teaser by the teasable is such as to naturally prompt the acts constituting the teasing, as the sense of burning makes us shrink, and an aroma suggestive of a fine flavor tempts us to bite. I feel convinced that the liability to be teased rests on a principle that has a mighty influence in the motions of the soul of humanity.

"My misdeeds, as a child, were rarely prompted by a love of mischief or the result of headlong thoughtlessness.

"I had a well-defined idea of the nature of sin, and I used frequently at night to recall the events of the day and reflect on instances in which I had transgressed and given way to ill-humor, and form resolutions to try and do better. From some of the most flagrant of the sins and improprieties to which small and larger boys are prone, I was entirely free."

Our patient, as has already been stated, was not a church member, and in his book he describes his early religious life and his subsequent beliefs as they developed. His father was a Universalist and his mother a non-professor of religion, although she did attend the Methodist Church. During his boyhood he attended Sunday School regularly, and at one time the Episcopal Church. But his attendance upon divine service ceased in early youth. Both parents were honest, conscientious, and highly respected in the

community. They were first cousins. The mother was healthy in mind and body, but the father is reported to have been exceedingly eccentric, possibly insane. From what I have recently learned regarding him, he also was something of a paranoiac. They strove to bring up their children carefully and educate them as well as possible. Concerning their inculcations in his youth, he says:

"My early training cannot be said to have been a predominantly religious one. My mind was neither imbued with ineradicable prejudices, nor prepared for reaction to the other extreme by excessively rigid sectarian drilling and formalism."

His father died when he was twelve years of age. Up to the age of thirteen he attended a country school, both winter and summer, but after that his farm-work permitted him only winter schooling. Still, he evidently had unusual talents and aptitudes, and we find him later studying by himself, in the original, many of the classic Latin authors; and among his favorite companions were the works of Boethius, Lucretius, and Josephus and the Bible. His literary style and modes of thought are in themselves an evidence of more than ordinary attainments in rhetoric, philosophy, and logic. I regret that I cannot present this book in full, because every page has its value as an index of the condition of his mind from childhood to the last years of his confinement in the asylum, and the story is told with a directness and simplicity that marks truth upon every statement and lends it such charm as pertains to all works which portray life with the utmost fidelity. In its way it has as much value as Amiel's *Journal Intime*, and its psychological analysis ranks higher, in my estimation, than some of the artificial work of Dostojewsky (Raskolnikoff).

The matter of heredity in his case was not sifted thoroughly upon his admission to the hospital, nor have I since been able to gather much material relative to this factor in his evolution. But one important element of this nature is described in his book—an element not only hereditary in its character, but for a long time part of his environment, and undoubtedly an influence modifying his mental condition both before and after his birth. I allude to a great-uncle, a brother of his grandmother on his mother's side, who was himself a paranoiac, and who lived upon the farm in intimate companionship with our patient until the latter was twenty-three years old. I cannot do better than permit the author to describe this peculiar personage and their life together, in his own words:

"I worked steadily upon the farm, though with moderation, at such kinds of work as I seemed to be equal to. The heavier kinds of work, such as plowing and wagoning, as also the marketing of the produce, were attended to by my great-uncle.

"It is a somewhat delicate subject to manage to my satisfaction, this that I am about to enter upon, but it demands candid and impartial treatment, because the events that followed in later years cannot be rightly understood without it. It is impossible for me to give a veracious sketch of my soul-life during this period without dwelling quite minutely on the characteristics of my great-uncle. He was a man who had roughed it a good deal in the world, had been at one time in his life a live-oaker in Florida. How his temper and disposition may have been at an earlier period I cannot say—I only remember him as a man possessed of the belief that a certain young man living on an adjoining farm had the power to torture him at his pleasure, both by bothering his brains and inflicting physical pain; which power he made use of to such good effect that the poor victim was almost constantly kept busy holding him at bay by means of cursings of the most fierce and vigorous description.

While at work with the horses in the fields, and when driving, he would intermix his commands to the animals with savage execrations of the troubler of his peace. The unfortunate man was troubled, at certain seasons of the year especially, with sore feet, and at such times his imprecations against the offender would fairly rise to yells, and were almost blood-curdling in their intense ferocity. Thus it went on day and night. He slept in a small room in one of the outbuildings, and often he could be heard a great distance off shouting out threats, sometimes throwing boots or boot-jacks against the boarded side of the building where he lodged, to put in the interjection points.

"It may be imagined that a boy of a reserved and sensitive disposition, as I was, could not assimilate very well with such a character as this. I was always distant in my intercourse with him, and a feeling of aversion for his habits of savagery led me to avoid coming in contact with him more than was rendered necessary by our joint labors on the farm.

"As the years passed on and I continued to live in the presence of my uncle's fierce demonstrations of hostility against the invisible destroyer of his comfort, my tolerance for his conduct insensibly gave way. I had now reached the age of eighteen or nineteen, was a tall slender youth, not strong either in nerve or muscle.

"The exhibitions of his ruling passion called up more and more determined feelings of antagonism in my breast.

"Before I knew it I had gone a criminal length in my resentful feeling. I came at last to feel that a person of such a thoroughly savage character did not deserve more indulgence than a mad dog. My position from that time was one of contingent murder. Alas! that I should have been content to let such a state of things last a single day. The frightful danger of my situation ought to have been sufficient to spur me to sacrifice everything to escape from it. But I was in chains, the chains of apathy, impotence and incapacity, and I could only stay where I was and fume against the object of my detestation.

"I must always regard it as one of the most unfortunate things in my unfortunate career that I should

have been placed in contact with this much to be commiserated sufferer at such a time of life. It was not the man himself that I hated. When my judgment could act without impediment, I saw that his unpleasant behavior was entirely the phenomena presented by his never-ending war against what was in his eyes the most wicked and cruel of persecutions. I could then pity him and dismiss all rancorous thoughts."

This antipathy led to a change in the residence of our author. He felt that he must be separated from his uncle, and accordingly he removed to a town at some distance from the farm. It is curious that he never speaks of his uncle as insane, and it is probable that both his mother and himself and other relatives regarded his persecutory delusions as merely evidence of eccentricity. Soon after removing to town he had some pulmonary difficulty, and he speaks at some length of this as follows:

"In the depressed state of my nerves I imagined myself much worse than I really was, and like many others in the same condition, I felt as if I was liable to sink away and die at any time. My disease was accompanied with periodical accesses of fever, and in the fictitious strength of excitement given by this, my mind seemed to gain an abnormal activity. It was at this time that I first received a revelation on the mysteries of the human soul that had an all-dominant effect on my destinies and the turn of my thoughts ever after. . . . I now learned what had always been to me a hidden mystery—what was the meaning of strength of will and strength of intellect. Before I had ever lived enshrouded in mists and clouds. In that transitory strength given by the fever coursing through my veins, I now saw the man I ought to have become rising up like a shadowy phantom in judgment on the wreck which I really was. . . . My agitation was so great that my mother and the neighbors seemed to fear I was going crazy. *I* felt that I *had* been crazy for a long while and had just recovered reason. It was a fact. But I was constrained to lock up my remorseful agony in my own breast."

We have seen that our patient was throughout his early youth morbidly subjective, and his hypochondriasis increased with years. He had now attained the age of twenty-three; we shall let him describe his mental condition and habits of life at this time. In this description we shall see the gradual growth of persecutory ideas upon a favorable soil:

"My strength and endurance were not sufficient for manual labor, and I did not feel confidence enough in the clearness and energy of my mind to justify me in making application for any post where head-work would have been demanded, or for which ready presence of mind, or a good address, would have been required. But it was the unpleasantness felt on contact with my fellow-men that operated more strongly than anything else in binding me down to the course of life to which I devoted myself. I felt my deficiencies most keenly every time I met a human being face to face. . . . I could not do otherwise than shun what was so galling to my sensibility, while appearing to conduce to no desirable end. . . . . But I am going to show that I still remained exposed to very great dangers, and it is as true as it was before that I shunned the only means of averting the calamities threatening me, no doubt of necessity at this stage, and in obedience to the eternal decree that every tree shall spread out and develop in accordance with the qualities given to it 'before it was in the ground.' I did not like the constraint imposed upon me by the presence of man. I did like the freedom of solitude. I strongly disliked many things I noticed in the manner and words of some I met, and there was nothing to prevent this dislike from occasionally being absorbed into my solitary musings, to find its final resolution in the passion of indignation in its various degrees of intensity as the case might be. I have spoken before of my defective means of defense against 'teasing' or mocking for the purpose of troubling. I was always terribly alert and sensitive to all kinds of 'snubs' and sneers, and oblique remarks in general, on their proficiency in which some people pride themselves so much. . . . . I was also disagreeably impressed by the ways of some

who showed a disposition to turn their attention to myself, instead of confining themselves to the subject I was presenting to them.

"I was being carried into a state of secret enmity to mankind in general by the prevailing tenor of my brooding meditations, and there was no corrective present.

"But all received a hue from a yearning for what was worthy in life, paired with a mournful sense of its hopeless absence. Whatever wrong turns I may in my weakness have been betrayed into, it is impossible that I should look upon my then existing frame of mind as a whole with repentant feelings. As well condemn righteousness and holiness itself!

"When I admit that I occasionally was overcome with an irruption of hard feelings toward wrong-doing man, it will of course not be understood that I was habitually morose and spiteful in temper. Nothing could be further from the truth. What commotion there was, was mostly internal, rarely reaching the surface in visible ebullitions. . . . I occupied myself with the trifling labors of my garden, dwelling with interest and pleasure on the progress of my crops and flowers, and every now and then took a ramble over to the woods lying to the south, which were a favorite place of resort to me all the while I lived there. There I botanized and moralized, explored the recesses of the woods, enjoyed the calm quiet of nature, and groaned over my hapless condition, wondering what it was to come to.

"There were some little things that happened to me the first year after I left the farm which became as it were a kind of sample of what I must continue to expect, and the memory of which had more influence over my action in aftertime than I was aware of myself, no doubt. . . . When I was around the city, thinking I might get employment I called on one of my old acquaintances, who was then in a store. I talked with him a few minutes at that time. I called again a short time after, when I was told by the proprietor that the gentleman I had called to see was not in. There were a number of men present in the store, salesmen, and it became apparent to me that they were trying to exhibit an offensive demeanor toward

me, or perhaps it would be as true to say that they were moved to make a derisive demonstration against me. At all events, all, with perhaps the exception of the proprietor, stood with contortions of countenance, which was perhaps laughter, until I retired. . . . I found it hard to consign this to forgetfulness. At first it lay dormant, but it would come up, and I must confess I had hard feelings, even revengeful feelings, toward the actors. Another thing happened the same fall. I went to a store and standing at the counter was noticed by one of the clerks, an Irishman, who came to me and said, 'I always wait on the little boys first,' and as I took no notice of the remark, seemed so determined his words should not be lost on me that he repeated them with the addition, '*like you*.' As before it produced no immediate effect, but it afterwards rose and rankled in my memory, and I was not able to keep clear of imagining vindictive things. In fact, to tell the truth, in both cases I felt that blood would have been sweet to me. . . . My mode of thinking on these incidents no doubt had in it much of the character of insanity. . . . The effect was that I got settled down into the fixed idea that contact with the thoughtless evil world, in my state of body and mind, would impose upon me the necessity of committing crime in vindication of my honor. . . . I let these bloody memories tinge my whole mind and all its anticipations and resolutions for the future. . . . 'I see,' I said to myself in substance, 'that these galling collisions are the natural penalties of being imperfect.'

"It may be as well, for the prevention of misconceptions, to say that I never took one step toward putting any design thence arising into execution. I had no designs. I never armed myself, or, in fact, went any further than to rehearse the drama of revenge in my own mind. The pistol I bought was one which I would not have trusted for a moment to carry for the purpose of self-defense. . . . . Nevertheless, the events on the farm show that my wickedness was not altogether of a mimic kind, and I will not attempt to escape righteous judgment.

"I used to make many resolutions about regularity in habits of eating which I found myself powerless to keep. A sense of depression and vacuity would come

over me, aggravated by my solitary, monotonous life, I presume, and often by an obstructed state of the alimentary organs. . . . . It is a common feature in insanity or semi-insanity left to itself, I think. I also exerted my brain to the extent of abuse, I know, in the way of study. . . . . I used to study Latin for a pastime, and often kept cudgeling my brains over Cicero and Cæsar until the top of my head was very sore. This solitary immersing of an enfeebled mind in study, with obliviousness to myself and all surroundings, was, no doubt, a help toward the grand consummation that took place in the fulness of things. . . . . I suffered a good deal from bodily ailments. My liver seemed to be thoroughly out of order and torpid. I had a feeling of hardness and inflammation in my sides regularly, a certain length of time after meals; digestion was bad, appetite irregular—in fact, every sign of a deadlock in the vital functions."

We have seen in these excerpts from the author's account of himself how heredity and environment had gradually moulded his physical and mental characters. A shy, timid, delicate child, clever intellectually, given to oddities of speech and conduct, inclined to solitary musing, rarely sharing the sports or games of other boys—in him were slowly evolving marked eccentricity of demeanor, a disposition to shun his fellows, a misinterpretation of their looks and actions as regarded himself, a morbid egotism, a consciousness of a gulf between himself and ordinary men, with deep depression, outbursts of passion, an inclination to homicide restrained but feebly by his weakened will, and delusions of persecution. No doubt the derogatory remarks he fancied expressed about him in the stores were the first harbingers of auditory hallucinations. Of late he had murder in his thoughts, through the morbid humiliation he felt at the imaginary insults from others. No doubt, as his conduct grew more and more strange, he did attract attention among his

fellow-men, and this, unfortunately, would but feed the flame of his pathological self-consciousness.

His mother and himself removed to another village in 1871, when he was 28 years of age, by which time there was but little question of his insanity, even among his relatives. I let him take the thread of the story again at this epoch:

"When my mother was making preparations for moving she asked me to help in packing up some chairs. I made an effort to apply myself to the task, but suddenly found myself overcome by my feelings, and before I knew what I was about I had shivered one of the chairs to fragments. A most unpromising omen! The fact is that I was and had been for some time in a state which any physician, knowing the facts, would have pronounced to be unmistakable insanity. But I had different ideas about what constituted insanity, and often thought to myself that if I did get put into an asylum, as had been threatened, they would not keep me, because they would see I was perfectly rational. I have learned more about the subject since.

"Things of the kind I have told of had happened to me before, at uncertain intervals, during several years, an obstructed state of the bowels bringing on a turn. I would get into such a condition of exaggerated discomfort as to lose for a moment, or sometimes quite a spell, my control over my actions and act very strangely. Sometimes I dashed down an article I happened to have in my hands, or demolished the first thing that came to hand; sometimes I gave vent to my feelings by grating my teeth, 'clawing' my face, and going through strange grimaces and agonizing contortions. My face seemed to me to be paralyzed when I had such turns, as if lifeless. The worst thing I ever did was when I flew at my mother in a sudden access of frenzy one day, when she had wrought upon my feelings by talking to me irritatingly, and bit out a mouthful of her hair. . . . . When I was committed to the asylum, at a later day, it was reported as one of my symptoms that I had delusions about my mother

being my enemy, etc., but nothing could be further from the truth. . . . . I often grieved in secret over my inability to be a stay and protection to her, bereft as she was of all other support, but all in vain.

"In my new home I was in one of a row of houses, with strangers living near on both sides, and the sense of the presence of the evil which I had shrunk from so long weighed down upon me with crushing weight. After awhile my spell of hypochondriacal despondency passed off, and I settled down into the way of living which I adhered to as long as I remained there. As to getting acquainted with my neighbors, or having any intercourse or dealings with them, that was altogether out of the question. . . I now had more of the feeling of constraint, from the knowledge that I was moving under the eyes of people who were stranger to me than the strangest of the strange could be to a person of the ordinary stamp. Sometimes I heard remarks which did not affect my feelings flatteringly, but that was not common.

"Along in June I had a worse spell than common of the kind of nervous stagnation or will-impotence of which I have spoken, and perpetrated some quite irregular acts before my fetters became slackened. In my despair I tore the collar from my shirt, tore the slippers I was wearing, dashed my fist into a tempting dish which my mother was offering me to eat, and other things of the kind. The house we occupied was owned by a maiden lady who lived with her sister in part of the house. . . . . In the evening after the other sister returned, who had been absent during the day, I overheard a few words which showed plainly enough that the events of the day were being discussed in no very gratified humor. It was evident that my acts were severely reprobated."

The next day the justice of the peace called upon him and admonished him to restrain himself, hinting of the asylum. Of this our author says:

"The dragon's-tooth of reprimand that had been left in my mind grew into a monster, in whose presence I found it impossible to live, and I had a fresh access of despair. It was a hot June morning. I re-

member seizing a razor and flourishing it, and saying, 'Show me that rascal and I will slaughter him,' or words to that effect, meaning, of course, the justice of the peace."

Both homicidal and suicidal inclinations had long been haunting the secret corners of his mind, for three years before he tells of buying a pistol for the express purpose of making way with himself or some one else. On this day, after meeting the officer, he determined upon suicide. He walked out to two different country stores and bought ammunition. On his way back he passed some men in a field. They all looked at him, and one of them "laughed loud and mockingly, and then cried out in a sort of squealing way, the intention of which could not be mistaken." Then he played a game of croquet with a young man at his uncle's, and overheard the young man make a covert and derisive remark. He continues:

"I passed the next day in brooding, silent, melancholy. It was a rainy day and in accord with my feelings. . . . . That night I wrote a little statement to be left behind. . . . . It cannot be said that I plunged thoughtlessly into the gulf of self-murder. I had from the first gauged the responsibility I was taking on myself, as fully as my mind was capable of doing it. I felt the whole weight of the condemnation that rested upon me for committing such a deed. . . . I passed some part of the hours of the night in sleep. In the morning my mother came to the door to see how I was, and I grasped her hand with a gesture of agonized despair. She took it as an indication that I was going to have one of my wild spells again, and, as she told me afterwards, began to anticipate some work of demolition after I should come down stairs. After she had gone down, I went and took the pistol from the stand-drawer, put on a fresh cap, got into bed again and propped up my head on the pillows, placed the muzzle of the pistol against the center of my forehead, and fired."

He lost considerable blood from the scalp-wound, but the bullet had glanced off; and, although he now tried to starve himself, he was up and about in a few days as usual, attending to his garden with bandaged forehead. He continues:

"There were some steps taken toward getting me into an asylum after my abortive attempt at suicide, but as there were difficulties about it, and I appeared perfectly sensible and rational, my relatives concluded to let it rest.

"From the time of my shooting until the next spring there was not much that deserves mention. How were my thoughts about suicide? It must be said that I had not totally renounced that idea. . . . . I used very often to scan the beams in the wood-house and the coils of clothes-line in the garret . . . . The old difficulty of giving way under the slighting or displeasing demonstrations from others remained as bad as ever. I remember once I was so wrought upon by some trifling thing said or done by one of my relations that I kicked out the bottom of a cane-seat chair I was resting my feet on, in a sudden paroxysm of impotent emotion."

About this time he also made a futile attempt to poison himself by drinking a bottle of strong tincture of valerian that he had made himself. This incident he describes, and then proceeds:

"It was my intention when I began this sketch of my life to give greatest prominence to that part beginning with my troubles in Clinton Street, that is to say, the period of confirmed lunacy with hallucinations, according to the world's avowed decision; but it appears at present that my project is not to go into fulfilment. I have been greatly delayed in doing as much as I have by lack of strength.

"To make the account which I have given as full an exhibition of my condition at the time my hallucinations (if such) appeared, I will note some further defects in my mental action which I had noticed up to

this time. First, two or three things indicating original lack of control over the brain by the will, or non-identification of my will with the action of my brain, and which I must count for predisposition. I have been troubled from my boyhood with a tendency of my brain to see things it ought not to see in what is placed before my eyes. This refractoriness does not extend to all kinds of monstrous visions, but is limited to the singling out of the lineaments of the human face in the outlines of objects seen. The annoyance I have experienced from this has varied greatly, according to the state of my health. When I used to be sick with the fever and ague, I would lie in bed and gaze at the coarsely-daubed window shades in my bedroom, until I had made out every possible kind of a profile that could be distinguished.

"The other of the two most serious abnormal peculiarities is the supplying of missing articulations to vocal sounds, heard but not understood distinctly, so as to give my mind the impression of certain words, at the same time that I knew I had not understood. Sometimes I have been really cheated this way, and only found it out by inquiring afterward. This might not give conclusive proof of the deception, it is true. Not to violate privacies, I will illustrate supposititiously. If it were proclaimed aloud, far enough from me to allow the inflections but not the articulations to reach my ear with certainty:

WE SEE WHERE LIES THE DREADFUL SECRET!

My mind might involuntarily and instantaneously reshape it in such a way that I would understand:

DECEIVE WHERE LIES WERE EVER SACRED!

"My attention was always quite easily disturbed by noises, particularly talking. In boyhood the sound of voices in conversation at a little distance after I had retired to rest often gave me very serious annoyance, showing excessive irritability of the brain.

"Such was my mental state on the eve of my being overtaken by a more marvelously awful fate than ever fell to the lot of mortal man.

"My original purpose was to follow the incidents having a bearing on my mental fortunes with tolerable minuteness, in an unbroken chain, up to the time of reaching that wonderful state in which I have existed for the last six and one-half years.

"I shall be obliged to confine myself more to generalities.

"I was in such a towering state of morbid sensitiveness, that a slight tinge of impertinence, brusqueness, or fancied contemptuousness, in the manner of those I met, put me on the rack at once. . . . It began to occur to me after a little that my ears were becoming wonderfully acute for such things. Very often I would hear lively discussions on my character, and disputes about the proper epithets and titles to be applied to me, which I understood perfectly at an astonishing distance off. . . . I was wrought up to such a pitch that I formed a resolve that if I were given a sufficiently open provocation, I would attempt a bloody revenge, and on one occasion went out with a razor in my pocket. . . . I had an oppressive feeling of impotence, as if paralyzed, and suddenly did things I had no intention of doing, as in breaking glass. . . . I had a soreness all through my limbs which I compared to molten fire running through my nerves.

"I began to hear responses to and comments on my performances, and it gradually dawned upon me that I had been making myself a conspicuous object of curiosity to the whole neighborhood. . . . The comments heard grew more numerous and more and more derisive. . . . I had no suspicion at the time of any of the inspiration being drawn directly from my head. I do not say it was so. This is the debatable ground. . . . It was not until about a week later that it became evident to me that I was hearing my own thoughts given expression to by foreign wills and voices."

There is a chapter or two of the volume given to illustrations of his hallucinations at this time, but, full of interest as they are, there is no space for their reproduction here. We have followed the author's history thus far from infancy through childhood and youth to manhood, and have seen how slowly but surely the hereditary seed sown in degenerative soil had taken root and flourished. His peculiar auditory acuteness, with his morbid shyness, soon gave rise to illusions of hearing, and these again were transformed

into hallucinations, as is evident if the thread of the narrative has been carefully followed. The curious foundation of his hallucinations he well illustrates and understands. An idea rises in his own mind of what people would say in discussing him, and immediately consciousness in the auditory area projects the idea in spoken words into the environment. He noted this peculiarity of his own thoughts being repeated to him by the voices about him, yet he could not correct the delusions to which they gave origin, but interpreted the matter with the reason and judgment of an insane mind. He naturally had the delusion founded upon his hallucinations that people were persecuting him, but upon this now grew another delusion. He began to believe that they could read and repeat his thoughts, that there was some magnetic means by which his tormentors could draw off his thoughts, that other wills could act upon his body, dominating his own will and causing him to do things he had no desire or intention of doing. I will allow him to continue in his own language:

"I heard a great deal about 'inducting,' 'conducting,' 'sphere of influence,' sometimes even 'poles,' positive and negative, and my brain was constantly compared to a magnet. . . . . I could find no better explanation myself for a long time than the theory of a fluid, similar to or the same as electricity, uniting brains."

Among the extraordinary tales he heard at this time was the following:

"One was the story of an English physician who had become acquainted with my magnetic properties, and who was on the spot at the beginning, directing the experiment. He was stated to have been the first to form a perfect communication with the inducted

brain, and he had drawn off my entire memory back to childhood, and had delivered it verbally in the presence of reporters from the city, who had taken it down. It was stated that the record was preserved in a number of thick volumes. These he had taken with him when he sailed for England during the most prosperous part of the experiment. It was further asserted that he continued in communication with my thoughts, and that wherever he went, every one to whom he told the story of the new marvel was also set in connection with the magnetic current flowing from my head, and began to participate in my thoughts. . . . . One word more of the English doctor. He is said to have declared that if he had assisted at my birth he would not have suffered me to remain alive, as the monstrous character of my organization could have been seen at a glance. . . . . After the whole earth had become pervaded with the magnetism from my head, it would be felt as long as I lived, and the instant of my death would be thus signaled all over the globe, and would be noted and used by all nations as a new era from which to reckon time."

It was about this time that he was removed to the asylum at Poughkeepsie. Several chapters of his book are devoted to a description of his life there, his religious beliefs, illusions and hallucinations, but from these I can make only a few extracts. A short time previous to his departure for the hospital he began to read much in the Bible. Of this he speaks as follows:

"I would think of the Bible, go and open it at haphazard, and just where my eye fell there was a passage that showed me *myself*. Once when I had been fretting about my ill success in getting my mother to accord with my views about my neighbors' doings, I hit upon this:

'And it shall come to pass that when any shall yet prophesy, then his father and his mother that begat him shall say unto him, Thou shalt not live; for thou speakest lies in the name of the Lord; and his father and his mother that begat him shall thrust him through when he prophesieth,' etc.—Zechariah xiii."

There were other similar coincidences, and he looked upon them at first as merely such, but in time the resemblances became so strongly marked to his disordered intelligence, that he came to look upon whole chapters of the Bible as referring to himself. He says :

"But the most perfect identity of all is to be found scattered through the Psalms," [of which he quotes several pages and then continues], "I do not intend to appropriate the spirit of these passages, or to make their language my own, but quote them thus collectively as an evidence of fact. I am myself but an enquirer. Do they express the experience of any certain person or persons? Or are they prophetic? . . . Can it be that the same thing that has happened to me has befallen another in ages long past, and that these are the traces of it?

"I have also found a most remarkably close application of many of the precepts and reflections of Thomas à Kempis in his 'Imitation of Christ.' He seems to keep the same character exhibited in the Psalms in view, only speaking as a monitor instead of in his person. I presume I find myself mirrored in both these places, because I am an extreme case."

He also found references to himself in Goldsmith's "Animated Nature," his own surname being there frequently used as an ordinary noun, although a word seldom employed in the English of to-day.

Gradually his delusions burgeoning one from another became so systematized that in the last year of his stay at this asylum he could write in his book :

"The signs are too many and too evident to permit me to doubt that my destiny is bound up with the religion of the world. I steadfastly believe that the words in Jeremiah, 'take forth the precious from the vile,' are addressed to me; and I cannot be recreant to the holiest of duties. . . . I will not waste time in useless discussion, but start with the assumption that it is God's will that I should give the world my opinions.

"If it comes to be generally believed that my sign is a fulfilment of Hebrew prophecy, I would recommend a transfer [of the Sabbath] to the day of the commandment. The very fact of a day one step removed being fixed on by both Christians and Mohammedans looks like an admission that another step remained to be taken.

"Was it not the confidence of Jesus in the book spoken of above that made him say he knew the Father, when contending with believers in personified derangement?"

Quite a large part of the volume is devoted to expounding the Scriptures, in accordance with his delusion that he is a prophet come to reveal a new religion.

For instance, of Babel he says:

"I find an application for the tower of Babel in my own insane history. I expect a confusion of the speech of the old sects to ensue likewise."

Of Abraham he remarks:

"Abraham is accounted the father of all who believe in the Eternal. I believe I am chosen as his sign for the abolition of all dishonoring beliefs, as Abraham was set up against all idolaters and pagans. . . . I have to note in connection with the offering of Isaac by Abraham, that I find the date given as 1872 before Christ, coinciding with the year after Christ in which my ear-troubles commenced."

Of Esau:

"We may take Esau for polytheistic religion, recognizing and deifying every force and passion that has dominion over the soul or destiny of man. . . . When it gave up its birthright for belief in a single judge, it pledged itself to go on and submit to be judged by the new master. I believe that the day of judgment has come."

Of the miracle of the rods:

"The rods changed into serpents signify arguments becoming living convictions in the mind of Pharaoh.

The evangelists' rods live as serpents in the minds of Christian believers, but I confidently expect that my rod will become a serpent that will swallow them all without trouble.

"Israel is held responsible for the destruction of the heathen and their idols. I conceive that I am the Lord's instrument for the completion of this work, and that I have been shown these signs in the law that my hands might be strengthened.

"I cannot shut my eyes to the fact that I have been made the world's sin-offering."

Of the prophets:

"The prophets I will take in a lump, with the assurance that no one can fail to see their connection with my destiny. There is a prophecy in Ezekiel, xxxiii. 30, which is very closely paralleled in my experience. . . . Jonah gives me a parable."

His discussions of theological questions are interesting, perfectly coherent and logical, although often fanciful. He pays tribute to the beautiful moral laws and righteousness of Christ, but is disposed to criticize his conduct as being inconsistent in one who claimed to partake of the omnipotence and omniscience of the Eternal. Of resurrection he says:

"If I conceive of a new body having the memory which I have of this body's life (and I can find no other idea of the continuance of a soul's life except in the perpetuation or renewal of the memory), would that in the new body be a *true* memory? Would it not be a hallucination? Would not that be an insane creation?"

We see from all that has thus far been quoted that our author had several incentives for writing this book. It contains the autobiography of a new prophet as well as the revelation of a new religion. From his standpoint as a man in whose destiny are wrapped up the destinies of the world, he tells posterity of the

tortures and trials he has passed through as an atonement for the sins of the earth, how he was mocked at and scoffed at, his brain acted upon by magnetic agency, and himself imprisoned in a lunatic asylum for years. Hence the title of his book: "*The Piling of Tophet and the Trespass-offering.*" But behind this insane egotism there shines at times some faint glimmer of the truth, so that he frequently speaks of himself, in the terms used by his fellows, as insane, a lunatic, a monomaniac, as having hallucinations; and he thinks the opinions of his friends, relatives and physicians, of sufficient worth to merit considerable argument in his book. He knows what insanity is; he recognizes it in his asylum associates. He could at times "see the man he ought to have become rising up like a shadowy phantom in judgment on the wreck which he really was." But this occasional consciousness of their disordered mental condition is by no means infrequent in the insane. In speaking of the years of his greatest mental aberration, he says:

"Here I come to more debatable territory, on which I and the rest of the world have until this present been at variance. I will, in deference to the other side, make use of the word *believe* in stating facts drawn from the region of my memory lying within this shadowy world. I will be permitted to say, therefore, that I *believe* that after settling down in the before-mentioned place, my brain was, by the gradual progress of events occurring naturally and according to the ordinary laws of human affairs, drawn into relations to the living actors around me, of an altogether unexampled kind—at all events different from anything plainly recorded in the annals of past ages. I *believe* that the final result of such relations was the superinducing of a state of mental intercommunication through the medium of my sense of hearing.

"But this is a very old story, and merely a re-state-

ment of the perfectly well known features of my alleged monomania. Let me pass on and give as well as I am able my own theory on which I explain these phenomena, which may have more interest. It is a question of personal identification. How does a man use his own brain? He can use it because it recognizes the actions of his members as belonging to the personal unit of which it forms the summit. Now the question is, cannot a human brain under certain circumstances become so perverted as to recognize for itself, and without the volition of its bearer, the acts of other individuals as belonging to its life, as falling within its own memory? And if so, would not those individuals become partakers of the intellectuality of that brain, know its conceptions and ideas, while it thus recognized their motions, and become able to share its walks and ways? Such I believe to have been the result in myself, from the towering height of disintegration reached by my mental organism, by the gradual process which I have endeavored to faintly shadow forth in the preceding five chapters.

"Let us see whether it does not look probable that a mind in the habit of separating recognized observations from its own responsibility, considering them objectively, philosophizing on its own manner of working, driving the impotent and erratically-acting part into a corner, as it were, would not be more exposed to such a fate as supposed than one acting unitedly, and right or wrong as a unit. It may not be susceptible of argument based on points of organic action, but it looks a plausible thing to me that the insane quality or element in such a brain might be acted on from without, and give itself up to such action, independent of the thinking will of that mind.

"But let us further suppose some little abnormality about the original constitution, a predisposition from a slightly dislocated arrangement of mind-apparatus and sense-apparatus.

"Such, say I once more, I believe to have been the case with myself, and such to be the true nature and essence of the things which have constituted my insanity. . . . . I do not deny the fact of insanity, but I firmly believe that it is and has been, since the summer of 1872, an insanity involving the will, ideas and acts of more than one individual.

"Notwithstanding my full and necessary faith in the reality of things as I have reasoned to prove them, I am still willing to concede that there has been more or less of purely subjective illusion mingled with these dual realities. Under one aspect the whole of this train of mental images and impressions which has whirled through my head has consisted of insane delusion. The effect on the state of my system has no doubt been analogous to that produced by delusions, and the nervous condition which preceded it was such as eventuates in the rise of delusions. Does not the development of delusions often have a compensating effect in freeing the nervous system in a manner from its trammels? Perhaps when this supervenes, the brain becomes a chimney for the combustion of the matters which threatened to entirely interrupt the action of the system by clogging. The patient is then known as sensible on most subjects, but a confirmed monomaniac."

Certain peculiarities in his hallucinations possess considerable interest. They almost always referred to the intercommunication of brains. In July, 1878, he wrote out a list of specimen phrases which he had heard while sitting alone at an asylum window. Some of these I reproduce here:

"One thing you know, you know when you get your will in there you get him into a hell of misery."—"He ain't got any will there to fool away."—"Although you are knowing his ideas you connect with her will."—"Instead of connecting with his ideas you keep giving him to her."—"You can't get your will there till he connects his thought to his thought."—"We are all the while trying to make him think himself."—"I think we ought to be making efforts to get the idea out on the hall."—"After they get the whole will he is in a hell of torture all the while."—"We keep hollering till we get him into a hell of horrors."—"You see, when there are two wills connected with the head at the same time, he ain't nowhere."

These were the voices of several men and women. In fact, his hallucinations were always polyphonic, and at times would be polyglot. They did not address him directly, but spoke to each other about him. He seldom had hallucinations of hearing, except when the ear actually received the sound of distant conversation or inarticulate noises; so that for their production it was usually necessary that there should be transmission of vibrations to the auditory cortical area. As instances of the polyglot character of the voices on occasion, I relate the following:

Once he heard some one call out, "If he ain't a prophet there never was a prophet—*tabulas dedi ut vincerer.*" In tracing this Latin to its source, he found it was a perversion of a phrase in a note to Whiston's Josephus: "*Egomet tabulas detuli ut vincerer*" (I myself carried the letter commanding that I be bound), attributed to Bellerophon, which he had once read.

At another time in a street car, a German sitting next to him cried out, "*Das ist das grösste Mirakel von der ganzen Welt. Jeder Gedanke der ihm in den Kopf gekommen ist hat die ganze Village gehört.*" (That is the greatest miracle in the world. The whole village has heard every thought that has come into his head.) The grammatical construction of the foreign phrases is open to criticism. The language used by his invisible tormentors was always a peculiar dialect, often abounding in slang, which he considered the most hateful kind of language, and which was such as he never voluntarily used in the composition of his own sentences. The hallucinations were usually boisterously satirical, teasing, quizzical, frequently accompanied with laughter.

It is seen that the author of "The Piling of Tophet"

was one of the most typical cases of a chronic primary insanity, as described by Morel, Sander, Krafft-Ebing, Mendel and others. Born with a constitution in which lurked a degenerative taint, he passed his youth as a hypochondriac, the victim of a morbid subjectivity, and on the eve of ripened manhood developed auditory illusions, which became slowly hallucinations, with the simultaneous evolution of delusions of persecution, followed later by systematized expansive delusions of a religious nature.

Shortly after writing his autobiography, he was removed to the county asylum at Mineola, where he remained without change in his mental condition until 1883, when his friends took him out to live with them. He spent eleven years in asylum life. He died a religious paranoiac in 1886. He did not become demented, but doubtless the indifference with which the world received the propagandism of the new prophet caused his philosophical withdrawal from active warfare in the fields of reform and theology.

MEMORY, HISTORICALLY AND EXPERIMENTALLY CONSIDERED.

W. H. BURNHAM, PH. D.

II.

Modern Conceptions of Memory.

I.—*Disciples of Hartley.*

The writer last noticed in this sketch of the conceptions of memory was David Hartley. Before passing to the Kantians and the Scottish School, an Italian philosopher should be mentioned, who, whether a disciple of Hartley or not, taught an associational psychology very similar to that of the great English psychologist. I refer to Francesco Maria Zanotti, whose little treatise, *Della forza attrativa delle Idee*, appeared under the false date of 1747, purporting to be the work of a Frenchman.[1] This work contains Zanotti's views of memory.

Zanotti agreed with Malebranche in considering memory as a kind of habit. In his opinion, it is not a faculty. A faculty is given us by nature; habit is acquired. Memory comes in this latter way. It is formed little by little, by the repetition of feelings and thoughts. While explaining memory by the association of ideas, he invented a theory, not found in Hart-

[1] See Opere di F. M. Zanotti, tomo V, Bologna, 1790; also Ferri: La psych. de l'association, ch. III

ley, to explain association. The attraction of ideas is the cause of association. What was little more than a figure of speech with Hume, became a psychological principle with Zanotti. He speaks not only of the attraction but of the electricity and magnetism of ideas, and he formulates the law that *the attractive force of ideas will be proportional to their fullness or perfection.* This attraction of ideas explains memory. According to Zanotti, "when we unite the idea of a certain thing with that of a certain time, these two ideas acquire by contact a kind of magnetism which causes one to attract the other; thus the force of reciprocal attraction is produced in the needle and the magnet when they have once been placed in contact. In the same manner, when the idea of a thing is awakened in us, it draws after it that of the time with which it has once been joined; and it is in this attraction that memory consists. Thus the thing reminds us of the time; and not less frequently the time reminds us of the thing. This is equally true of place; for the memory of a place brings back to us that of the event which happened there, and that of the time in which it occurred. These ideas of thing, of time, and of place having been once united, have acquired a sort of friendship—have become, so to speak, magnetic, and have begun reciprocally to attract one another."[1]

Among other disciples of Hartley may be mentioned Priestley and Erasmus Darwin. The last mentioned distinguishes ideas that we voluntarily recall as "ideas of recollection," those that recur involuntarily as "ideas of suggestion."[2] He argues vigorously for the

[1] Ferri, op. cit., p. 58.

[2] Zoonomia, Vol. I, §II, 2, 10.

theory that recollection is reproduced movement. He says: "If you wonder what organs of sense can be excited into motion when you call up the ideas of wisdom or benevolence, which Mr. Locke has termed abstracted ideas, I ask you by what organs of sense you first became acquainted with these ideas? . . . . If our recollection or imagination be not a repetition of animal movements, I ask, in my turn, What is it? You tell me it consists of images or pictures of things. Where is this extensive canvas hung up? or where are the numerous receptacles in which those are deposited? or to what else in the animal system have they similitude?"[1]

Abraham Tucker, another disciple of Hartley, deserves mention; for, while holding that an idea is a reproduced movement or cerebral modification of some kind, he avoids some of the objections usually brought against a physical theory of memory. He does not think that the motion or modification caused by our seeing an object persists until we see the same object again, perhaps a year afterward. Such a view would render necessary the supposition that "our internal organs must be as numerous as the ideas we possess." "But," he argues, "one substance may be susceptible of various modifications at different times, and as the same optic nerves serve to convey red, yellow, or green, according to the rays striking upon them, so the same internal organs may exhibit various ideas according to the impulse they receive from elsewhere."[2] That is, as I understand the passage, the

[1] Op. cit., §III, 6 & 7.

[2] Human Nature, Ch. VIII, §3. But see also §2, where he finds the analogue of memory in the visual after-image, and says that "our mental organs have a like quality with the bodily, of conveying perception to the mind when the causes setting them at work no

same nerves have the power of vibrating in different ways as the external stimuli vary. In like manner, the same nervous substance of the brain has the power to reproduce different vibrations as the stimuli vary; the stimuli in the latter case being, of course, either the sensations from external objects or associated ideas.

II.—*Kant and His Disciples.*

Kant's views of memory are presented at some length in his later writings.[1] But, indirectly, much is said about memory in the Critique of Pure Reason, in his discussion of the relation of the imagination to knowledge.[2] It is not necessary to present Kant's theory of knowledge here, but it will be remembered that the imagination mediates between sense and the understanding, partaking of the nature of both. It is allied to sense because we can imagine only sense-perceptions; it shares the nature of the understanding because it synthesizes the manifold of intuition according to the categories, and this synthesis is an exercise of the spontaneity of the mind. Kant distinguishes two forms of the imagination—the *productive* and the *reproductive*. So far as the imagination is an exercise of the spontaneity of the mind, clothing the pure concepts of the mind in sensuous form, he calls it *productive*. So far as it merely revives past sense impressions it is *reproductive*. The former acts according to universal and necessary laws; the latter, according to the empirical laws of association. The former may go

longer operate. For what the impulse of objects is to the optic or auditory nerves, that the impulse of these latter is to the mental organs; yet we see the idea of an object may be retained after both those impulses are over."

[1] See Anthropologie, §§27-33; also, Pädagogik, A.

[2] See the Transcendental Logic, passim.

beyond experience in the presentation of pure intuitions of space and time; the latter implies previous empirical perception. The former is the imagination of the poet and the painter; the latter, the ordinary power of any Gradgrind. The relation of the imagination to what is often included under memory is evident from Kant's definition of the former. "*Imagination*," he says, "is the faculty of perceiving an object without its presence."[1] He distinguishes memory from the reproductive imagination by limiting the former to the *voluntary* reproduction of a presentation. The productive imagination, he says, if mingled with memory, renders it false.[2] The highest act of memory, that of recognition, is, in Kant's psychology, a transcendental act of the ego.

Kant distinguishes three modes of memorizing—the mechanical (*mechanisch*) method, the method of clever devices (*ingeniös*), and the method of reflection (*judiciös*). "The first consists merely in frequent, literal repetition." The second is a method of learning given ideas by associating them with others, however incongruous, thus burdening the memory by the arbitrary association of disparate ideas. The third method is the best. It consists in classification, and will enable one by reflection to recall what is wanted.[3] Kant has no especial praise for the mnemonic art, and memorizing for mere discipline he deems useless. But in the Pädagogik he gives some advice for cultivating a child's memory.[4]

[1] Kritik der reinen Vernunft, Supplement XIV: "Einbildungskraft ist das Vermögen, einen Gegenstand auch ohne dessen Gegenwart in der Anschauung vorzustellen."

[2] Anthropologie, §33.

[3] Loc. cit.

[4] Pädagogik, Sämmt. Werke, Vol. IX, p. 403 (Rosenkranz-Shubert ed.).

Fichte develops the theory of memory involved in Kant's doctrine of the imagination.[1] According to his idealistic psychology, the power of imagination arises through the development of freedom in reflection. The imagination has the free power to renew external perceptions. In fact, we might say that the imagination is this power of renewing perceptions, for further activity of the imagination would be merely idle play. The possibility of reproduction of perceptions by reflection is always present. But this possibility is merely a norm of thought, according to which a perception would be actually reproduced. The gist of this norm is to direct the imagination how to limit itself self-actively in its survey of the whole field of reproducible perceptions. Such reproduction implies classification and keenness of sense-perception. The external senses do not give us the renewed perception, hence the imagination must have the power by its own causality of renewing the different elements of the original perception. The immediate perception differs from the reproduced picture in this: the latter is always accompanied by the consciousness of self-activity. The ego feels that it makes every feature of the picture. The actual perception, on the other hand, is always accompanied by the consciousness of lack of freedom. The act of reproducing a perception is, to quote Fichte's words, "a self-limitation of the imagination within its whole field in accordance with a limitation of the outer sense. The norm (*Regel*) of this limitation is the concept, namely, of the object of

[1] See Die Thatsachen des Bewusstseins (Vorlesungen, 1810–11), Ch. IV, and Zweiter Abschnitt, Anhang über das Erinnerungsvermögen. Also Die Thatsachen des Bewusstseins (Vorlesungen, 1812–13), Vortrag IX.

the external perception which is reproduced."[1] Fichte distinguishes two cases of the reproductive imagination, namely, when the reproduced impression is joined with the concept, and when it is not. In the latter case we have *unconscious reproduction;* in the former, *remembrance.*[2]

The aim of reproduction of perceptions is to get the external world independent of itself in our power. Henceforth we are free in reference to the world. We may let its stream flow or stop it at will. Hence the importance of cultivating the memory.

Fichte was always at his best when he left the transcendental heights of his philosophy and talked about practical matters. In his chapter upon the reproduction of impressions, he does not fail to add some good practical suggestions. It is well to have the parts of our free self-built world in a firm and lasting form. The imagination left to itself easily becomes confused. It must be limited. *Writing* is the means by which we may bind our wandering thoughts. If our thoughts are vague, we shall notice it when we write them down or when we test what is written.[3] For making the reproduction of visual impressions definite, Fichte also recommends *drawing.* Practice is the great means of strengthening the power of recollection. Fichte even says that "special natural endowment, talent, genius, or whatever we may call it, has no influence" on this power. The power of recollection is not an accidental phenomenon; "but it is a necessary and inseparable part of consciousness." Hence

[1] Thats. d. Bewusst. (Vorles. 1810–11), p. 43.

[2] Thats. d. Bewusst. (Vorles. 1812–13), Nachgelassene Werke, p. 494.

[3] His aphorism is: "Ein gründliches und durchgeführtes Denken wird, meines Erachtens, kaum anders gelingen, als mit der Feder in der Hand." Loc. cit., §9.

anything that benefits the mind is good for the memory. "The clearer, freer, and more self-controlling consciousness is in general, the more comprehensive and ready is the power of recollection. (The true principle of a system of mnemonics is the maxim, *sapere aude*)."[1]

Schelling has nothing to say upon the subject of memory that need detain us. F. A. Carus,[2] however, a disciple of Kant whose writings are little known in this country, treats the subject of memory more fully and, in some respects, more satisfactorily than either Kant or Fichte. He holds an idealistic doctrine analogous to the doctrine of physiological psychology, that all our experience modifies the brain-centers in their growth. Memory means continued assimilation. Everywhere in nature, action implies reaction. In the subjective world this reaction, according to his view, is an effect persisting in time. "The continued life of man is, however, an advance from an original state of indefiniteness and dependence to a condition of greater and greater definiteness and stability. . . . Hence the ability to appropriate experiences, changes, developments, and what is received from sensation or feelings, and to cause it to persist as one's inalienable inner possession. Even this ability may be looked upon as the condition of memory, if not as memory itself. What has been experienced continues to live in us. But nothing that persists in us enters the sphere of our consciousness, consequently not into the higher sphere of activity of the mind and will, nor into our free power, without continued activity; thus what has been experienced does not enter consciousness

[1] Anhang über das Erinnerungsvermögen.

[2] See his Psychologie, Bd. I, p. 217 seq.

without reproduction, without being made our own by repetition." Memory is not a mere power of receiving and of preserving impressions; "it is rather a power of making present and living what has been perceived, thought, felt, and willed, together with more or less of a revival of space or of time." Finally, memory is not a separate faculty, but is involved in the other powers of the mind.

Hegel's doctrine of memory is so interwoven with his metaphysical system that space cannot be taken for a presentation of it here.[1] But what has been said of Fichte's views is sufficient to show the general character of the German idealistic psychology of memory. The most important feature of it is the emphasis placed upon the *spontaneity* of the mind in memory. One other important part of this doctrine, however, must not be omitted.

How the mind can preserve ideas has been the problem that philosophers have wrestled with or ignored ever since the days of Plato. Some of the German idealists attacked the problem and attempted to explain this mystery of memory; or rather, like Diogenes, they reversed the problem and attempted to explain forgetting. Fries, professor at Heidelberg before Hegel, is a good exponent of their views.[2] He maintains, with Locke, that there is an internal sense that takes cognizance of our mental activity. He adopts the distinction usually made by careful writers, between memory and the power of recollection. Memory, according to his definition, is merely the power of

[1] For a discussion of the subject from the Hegelian standpoint see Rosenkranz's Psychology, pp. 267–327.

[2] See his Neue Kritik der Vernunft, Bd. I, §§29–36; Psych. Anthropologie, Bd. I, §§ 10, 11, 43; Logik, §§ 10, 132. Cf. the views of Carus given above.

preserving impressions that we have once had. Provided we do not mix the physical and the mental, it is his opinion that pure introspection will show that there is no difficulty in the theory of memory. Every cause in nature, once set in action, continues until checked by other causes. This law of inertia holds in the mental world as in the physical. Hence, "not memory and the preservation of presentations that have once been had, but the forgetting of them, requires special explanation."

To solve the problem of forgetting, we must notice the distinction between our clear and obscure ideas, or the relation of the inner sense to memory. Only the most lively activities affect the inner sense sufficiently to appear in consciousness; the quietly continuing changes remain for the most part obscure. "Hence we always notice our memory only in its influence on the internal perception of presentations. All ideas that we once have or have had, remain present to us; the whole stock of our presentations remains in the memory. But, as new impressions are continually pressing in upon the old and sharing the presenting power of the ego, this becomes more and more divided, the previous presentations are thrust back and very soon obscured. Hence, if earlier presentations are not from time to time renewed they gradually become weaker and weaker. In this manner we must conceive that our whole knowledge is always present in the mind, but that at each moment only very few presentations have sufficient clearness to reach consciousness, *i. e.*, to be perceived. Hence we can regard forgetting as only a gradual obscuring of our presentations, without needing to suppose that they are ever entirely lost to us."[1]

[1] Neue Kritik der Vernunft, Bd. 1, §30.

Recollection, or the return of obscured presentations to consciousness, according to Fries, is not properly a reproduction, but a reappearing. The two causes of recollection are an increased sensibility of the inner sense and an increased vividness of ideas brought about by association. In the phenomenon of association we find one idea so bound with another, that if one becomes clear again, the other grows clear also. "The law of the strengthening of one presentation by others, with which we here have to do, is the opposite of the law of the weakening of presentations that we found in the study of memory. Both rest on the meeting of presentations in one life-state, and in their affinity in respect to their origin. If one presentation meets another, then it weakens it, since both must share the mind's power in an equal degree. But if they have previously been together, and one is again strengthened, then it imparts this strength also to the other. . . . The different activities which the mind manifests at the same time belong to one act of the mind; the strengthening of a part of this activity is at the same time a proportional strengthening of this whole activity; that is, inner activities affect each other more or less by the law stated, according as they belong together in one action of the mind."[1]

This doctrine of memory was afterwards taught by H. Schmid,[2] and was adopted by Sir William Hamilton.[3]

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[1] Op. cit., §33.

[2] Versuch einer Metaphysik der inneren Natur, pp. 231–235. Quoted by Hamilton, Lectures on Metaphysics, L. XXX.

[3] Works, loc. cit.

III.—*The Scottish School.*

The views of Hume have already been noticed. Among other early Scotch writers who have something to say of memory or of the association of ideas are Hutcheson, Turnbull, and Henry Home. But the first representative writer of the Scotch School is Reid. He discusses memory at some length, but does not throw much light upon the subject.[1] "It is by memory," he says, "that we have an immediate knowledge of things past." In his opinion, "memory is an original faculty, given us by the Author of our being, of which we can give no account but that we are so made." The distinguishing feature of memory is that it implies belief in what we remember. Yet, sometimes "the memory of a thing may be so very weak that we may be in doubt whether we only dreamed or imagined it"; and children may be subject to illusions of memory. "Our notion of duration, as well as our belief in it, is got by the faculty of memory."

In discussing the subject of memory, Reid made some statements that are astonishing to most philosophers. "The knowledge which I have of things past, by my memory," he says, "seems to me as unaccountable as an immediate knowledge would be of things to come." He thinks it remarkable that men have found difficulty only in reconciling prescience with free acts. He maintains "that we can as little account for memory of the past actions of a free agent. If any man thinks he can prove that the actions of a free agent cannot be foreknown, he will find the same arguments

[1] See Essays on the Intellectual Powers of Man, Essay III.

of equal force to prove that the past actions of a free agent cannot be remembered."[1]

Reid maintains that association and vividness of ideas cannot account for memory. He appeals to consciousness to decide the point. "I could as easily believe," he says, "that a hat is a pair of shoes as that memory is a certain degree of vividness in ideas and of strength in their association." "A malefactor that is going to be hanged has a cluster of very vivid ideas, and very strongly associated, of what he is about to suffer, but it is not the object of remembrance but of foresight."[2]

James Beattie, a contemporary of Reid, wrote a popular dissertation upon memory, in which he repeated the usual stories and platitudes, and gave some excellent advice in regard to training the attention and the memory.[3]

None of the Scottish philosophers have described the facts of memory better than Dugald Stewart. He presents a good analysis of the subject as studied introspectively, describes the different varieties of memory, gives a collection of cases of remarkable memory, and discusses at length the means of improving it.[4] While he confines himself chiefly to the discussion of the well-known phenomena of memory and to the presentation of facts, he modestly presents a theory of the relation of memory to conception that contains the main features of the doctrine of memory developed more recently by Taine.[5]

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[1] Loc. cit., pp. 341 and 342, Hamilton's edition.

[2] MSS. papers by Dr. Reid, cited by Dr. McCosh. See Appendix, Art. III, pp. 473 and 474 of McCosh's Scottish Philosophy.

[3] Dissertations Moral and Critical, Dis. I.

[4] See Elements of the Philosophy of the Human Mind, Vol. I, Ch. V and VI.

[5] Intelligence, Part II, Book I, Ch. 2 and passim.

Conception, in Stewart's psychology, is "that power of the mind which enables us to form a notion of an absent object of perception, or of a sensation which it has formerly felt." In opposition to the generally received doctrine of his day, Stewart maintains "that the exercise both of conception and imagination is always accompanied with a belief that their objects exist." In most cases this belief is only momentary, and is "immediately corrected by the surrounding objects of perception."[1] When we recollect a past event in which an object of sense was concerned, the act of recollection involves an act of conception. But the former implies a belief in the past existence of the object; the latter, a belief in its existence at the present moment. How shall we reconcile this apparent contradiction? "The only way that occurs to me of removing this difficulty," says Stewart, "is by supposing that the remembrance of a past event is *not a simple act of the mind*, but that the mind *first* forms a conception of the event, and *then* judges from circumstances of the period of time to which it is to be referred. . . . So long as we are occupied with the conception of any particular object connected with the event, we believe the present existence of the object; but this belief, which in most cases is only momentary, is instantly corrected by habits of judging acquired by experience."[2] This reference of recollected events to the proper points of time in the past is "strikingly analogous to the estimates of distance we learn to form by the eye."[3]

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[1] Op. cit., Ch. III.
[2] Op. cit., Ch. VI.
[3] According to this view, there are three operations in any complete act of memory: (1) the reproduction of a previous state of consciousness—*i. e.*, an illusion; (2) the rectification of this illusion by the present state of consciousness; (3) localization in the past. See Taine, op. cit.

Thomas Brown, Dugald Stewart's successor at the University of Edinburgh, writes with his usual eloquence of memory. The main point that he brings out is that "memory is not a distinct intellectual faculty," but that "the state of mind in memory is a complex one," resulting from the combination of two elements, namely, a conception and a felt relation of priority. Moreover, he analyzes what he terms voluntary recollection, and shows that it, "whether direct or indirect, is nothing more than the coexistence of some vague and indistinct desire with our simple trains of suggestion."[1] He discusses the time-honored question of the relation of a good memory to intellectual power, and maintains that "it is not a good memory in its best sense, as a rich and retentive store of conceptions, that is unfriendly to intellectual excellence, poetic or philosophic, but a memory of which the prominent tendency is to suggest objects or images which existed before, in the very order in which, as objects or images, they existed before, according to the merely imitative relations of contiguity." Thus the difference between the ordinary man and the genius is, that in the mind of the former, ideas follow each other mainly according to relations of former contiguity, which are limited, while in the mind of genius they are suggested by the relations of analogy, which are infinite.[2]

Sir William Hamilton, the Hercules of the Scottish school, discusses memory and the association of ideas with his usual profoundness and display of erudition.[3]

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[1] Lectures on the Phil. of the Human Mind, Lect. XLI.

[2] Loc. cit.

[3] Lectures on Metaphysics, Lect. XXX, and Notes D** and D***, Ed. of Reid, Vol. II.

Yet his discussion presents little that is new. In the first place, by keen analysis and clear definitions, he avoids the confusion that is liable to arise in the use of the word memory.[1] He uses the word (as the most careful philosophers have done before his time and since) to denote merely retention. He has no good opinion of physiological hypotheses for explaining memory. "All of them are too contemptible even for serious criticism," he exclaims with dogmatic impatience. Nor are they necessary. The phenomena of memory present no difficulty. Forgetting is the puzzle. Here he quotes with approval from H. Schmid the theory of memory explained above in outlining the views of Fries. This theory of the persistence and obscuration of ideas, and his famous law of association by *reintegration*, are the most important features of his doctrine of memory and recollection. It is not necessary to repeat the former here; it is apart from the plan of this article to discuss the latter.

[1] In the following passage he points out the cause of the confusion in the use of this word: "Because the faculty of Conservation would be fruitless without the ulterior faculties of Reproduction and Representation, we are not to confound these faculties, or to view the act of mind which is their joint result, as a simple and elementary phenomenon. Though mutually dependent on each other, the faculties of Conservation, Reproduction, and Representation are governed by different laws, and in different individuals are found greatly varying in their comparative vigor. The intimate connection of these three faculties, or elementary activities, is the cause, however, why they have not been distinguished in the analysis of philosophers, and why their distinction is not precisely marked in ordinary language. In ordinary language we have indeed words which, without excluding the other faculties, denote one of these more emphatically. Thus in the term ***Memory***, the Conservative Faculty—the phenomenon of Retention—is the central notion, with which, however, those of Reproduction and Representation are associated. In the term *Recollection*, again, the phenomenon of Reproduction is the principal notion, accompanied, however, by those of Retention and Representation as its subordinates." Metaphys., Lect. XXX.

IV.—*The English Associationists.*

The English associational psychology, as already indicated, began with Hume and Hartley, while others, notably Hobbes and Locke, had discussed the phenomena of association long before the appearance of the "Observations on Man." The Scottish philosophers also gave attention to the subject, and Thomas Brown might be classed with the associationists, so much importance does he give to what he calls the mind's capacities of Simple and Relative Suggestion.

The real successor of Hartley, however, was James Mill, and the latter treats memory at length.[1]

Mill analyzes the phenomena of memory carefully but not quite thoroughly. He distinguishes two cases —the remembrance of sensations and the remembrance of ideas. In the remembrance of a visual sensation, for example, there is the idea of a thing and that idea brought into the mind by association. "But in memory there is not only the idea of the thing remembered; there is also the idea of my having seen it. Now these two—1, the idea of the thing, 2, the idea of my having seen it—combined, make up, it will not be doubted, the whole of that state of consciousness which we call memory."[2] The last part of this compound is a very complex idea containing two elements—"*the idea of my present self*, the remembering self; and *the idea of my past self*, the remembered or witnessing self." In the moment of memory, "the mind runs back from that moment to the moment of perception. That is to say, it runs over the intervening states of consciousness called up by association.

[1] Cf. Analysis of the Phenomena of the Human Mind, Vol. I, Ch. X.
[2] Loc. cit., p. 329, J. S. Mill's ed.

But 'to run over a number of states of consciousness called up by association,' is but another mode of saying that 'we associate them'; and in this case we associate them so rapidly and closely that they run, as it were, into a single point of consciousness, to which the name MEMORY is assigned."

Thus the recognition of a *sensation* consists of "three principal ingredients: 1, the point of consciousness called the remembering self; 2, the point of consciousness called the percipient self; 3, the successive states of consciousness which filled up the interval between these two points." In remembering an *idea* the ingredients are the same, except that the second is the "conceptive self."

The phenomena of forgetfulness confirm this account of memory. "Every case of forgetfulness is a case of weakened or extinct association. I cannot remember the discourse that I learned years ago, because the few words that I recall fail to suggest the following words."

Illusions of memory occur when "a case of the memory of *ideas* comes to be mistaken for a case of the memory of *sensations*." This occurs to the liar, when, in continually repeating his story, he dwells lightly upon the idea of himself as fabricating it and strongly upon the idea of himself as an actor in it, until, finally, the strongly associated idea of the latter circumstance overpowers and destroys the weakly associated idea of the former.

To sum up our account in Mill's own words, "Remembering is associating." And memory "is an idea formed by association of the particulars of a certain train—a train of antecedents and consequents, of which the present feeling is one extremity."[1]

[1] Op. cit., Vol. II, Ch. XIV, §5.

The way in which Bain and Spencer have developed the associationist psychology and filled the gaps in the earlier form of it is too well known to be recounted here.[1] It is necessary to recall only a few important features of their doctrine of memory on its physiological side.

We have already had occasion to note the anticipations, notably by Bonnet and Erasmus Darwin, of Bain's famous doctrine, that when an impression is reproduced, "the renewed feeling occupies the very same parts and in the same manner as the original feeling, and no other parts and in no other assignable manner."[2] This, argues Bain, is the only view compatible with our knowledge of the nerves. In the case of the after-impression of sense, we must infer that the continuing sensation is the result of persistence of the nerve-currents aroused by the original stimulus. If impressions surviving their originals are due to persisting nerve-currents, it is likely that revived impressions are likewise due to re-induced currents, feebler but in the same nerve-tracks as were occupied by the original sensation. Observation confirms this doctrine. The recollection of language is suppressed articulation. The vivid thought of an action impels us to perform it. Lively imagination of a color fatigues the nerves of sight. The thought of laughter keeps the hysteric laughing; and the hypnotized patient acts out all ideas suggested to him.

The physical mechanism of retention is as follows, according to Bain: "For every act of memory, every

[1] For a valuable presentation of their doctrines, see Ferri: La Psych. de l'Association. Also for valuable additions and illustrations, see Hodgson's "Theory of Practice," Vol. I, B. I, Ch. III, and an article on "The Association of Ideas," by Professor James, in Pop. Sci. Monthly, March, 1880.

[2] The Senses and the Intellect, p. 338.

exercise of bodily aptitude, every habit, recollection, train of ideas, there is a specific grouping or co-ordination of sensations and movements, by virtue of specific growths in the cell-junctions."[1]

As retention thus depends on a physical process, it follows that acquisition is limited by the size of the brain. "We are all blockheads in something." Great capacity in one branch of education is apt to be purchased at the price of corresponding deficiency in something else. Yet, from a rough estimate, Bain compares the number of our acquisitions, motor as well as sensory, with the number of cells and fibers in the brain, and concludes "that there is no improbability in supposing an independent nervous track for each separate acquisition."[2]

Herbert Spencer studies the genesis of memory.[3] In the growing complexity of the adjustment of inner relations to outer relations that, according to him, constitutes the evolution of life, there comes a stage when there are only "fragments of correspondences" between the two. At this stage memory appears. In instinct, the process of adjusting internal relations to external relations is complete; in memory, there are at most only partial correspondences. Yet the germs of memory are found in instinct, *i. e.* in compound reflex action. For the nerve center receiving and co-ordinating a series of impressions cannot record all these impressions at absolutely the same instant; yet, as the appropriate reaction is a response to the whole group, it follows

[1] Mind and Body, p. 91, 2d ed.

[2] For details of his estimate, and for the way in which he supposes the nerve groupings arise and can be isolated, see Mind and Body, p. 96, seq. See also his article on "The Retentive Power of the Mind," Fortnightly Review, Sept., 1868.

[3] Psych., Vol. I, Part IV, Ch. VI.

that the first impression of the series must persist until the last is received. This persistence is weak memory.

Furthermore, when psychic states have been infrequently connected in a series, their cohesion is feeble and the transition from each state to the next in the series is slow—*e. g.* in learning a language. This deliberate succession of psychic states is one of the conditions of memory.

Again, it often happens that two groups of impressions, differing by only one or two elements, are responded to by very different reactions; (*e. g.* two animals that look alike, one of which retaliates when attacked and the other does not. Here the groups of impressions from the two animals are, for the most part, alike; but the appropriate reactions are very different, *i. e.* running away in the one case, attack in the other). When these particular groups occur infrequently, the appropriate reaction becomes undecided. For the common elements of the two groups tend to excite either of the two sets of action that have constituted the responses to them. Hence arises a conflict between the psychic states involved in the two movements, and they become merely nascent movements or tendencies. This is the beginning of memory. To quote Spencer's words: "In the chief nervous center the different impressions serve as different motor impulses; and these, being severally supplanted by one another before they pass into the actual motor changes, will each of them consist of an incipient or faint form of that nervous state which would have accompanied the actual motor change had it occurred. But such a succession of states constitutes *remembrance* of the motor changes which become incipient—

constitutes a *memory*. To remember a motion just made with the arm is to have a feeble repetition of those internal states which accompany the motion—is to have an incipient excitement of those nerves which were strongly excited during the motion. Thus, then, these nascent nervous excitements that conflict with one another are really so many ideas of the motor changes which, if stronger, they would cause; or rather they are the objective sides of those changes which are ideas on their subjective sides. Consequently, memory necessarily comes into existence whenever automatic action is imperfect."

Moreover, the sensory memory is a concomitant of the motor memory just described. As the external groups of relations become more complex and infrequent, the corresponding groups of impressions become less coherent, and a nascent memory of the component parts of a group becomes possible. General sensory memory is a derivative from this; "for the same progress which gives the ability to receive the complex impressions required to determine complex actions, gives the ability to receive complex impressions which do not tend to determine any actions at all." The latter class of impressions having no direct connection with action, have direct connection with each other in varying degrees of constancy, and tend to arouse one another in varying degrees. Hence arises that succession of ideas we call memory.

While "memory comes into existence when the involved connections among psychical states render their successions imperfectly automatic," the obverse of this is true; and when by multiplied experience the appropriate response to outer relations becomes structurally registered, then conscious memory passes into

unconscious or organic memory. Thus, while "instinct may be regarded as a kind of organized memory, on the other hand, memory may be regarded as a kind of incipient instinct."

V.—*Herbart.*

Turning back to Germany, Herbart and Beneke are the first prominent representatives of the "new psychology."

In the psychology of Fries we have already seen foreshadowed that theory of the inter-relation and ceaseless rise and fall of presentations over the threshold of consciousness that constitutes the static and dynamic of Herbart's psychology. But Fries emphasized the spontaneity of the mind, Herbart its receptivity. According to Fries, a presentation returns to consciousness because strengthened by another; according to Herbart, because an antagonistic presentation is arrested.

According to Herbart,[1] what occurs in the constant meeting in consciousness of new presentations with the old is this: similar presentations blend; contrary presentations mutually arrest each other; disparate presentations are complicated. But even arrest does not involve annihilation. The weaker presentations are obscured, but they persist as tendencies, and as soon as hindrances are removed they become actual; for presentations, by their very nature, are always striving for self-preservation. Reproduction is either immediate or mediate. It is immediate when presentations reappear in consciousness merely by reason of

[1] Cf. Lehrbuch zur Psych., passim; also, Psych. als Wissenschaft, passim. For a good account, in English, of Herbart's psychology, see also articles by G. F. Stout, in Mind, Nos. 51 and 52.

the removal of hindrances, and without help from presentations that have previously been blended or complicated with them. It is mediate when a presentation reappears in consciousness by the help of other presentations previously combined with it. A series of presentations is reproduced in consciousness in the same order in which it was originally perceived, for this reason: each successive member of a series blends with all the other members of the series that are above the threshold of consciousness, with an energy proportional to the vividness of the latter. For example, in series α, β, γ, δ, suppose that the presentive activity of β is at a maximum and α is sinking, when γ appears: then γ will combine closely with β, lightly with α. When δ appears, the presentive activity of γ is at a maximum, β is sinking, and α is fading from consciousness; consequently δ combines closely with γ, lightly with β, very lightly with α; conversely α, which combined closely with β, combined lightly with γ, and very lightly with δ. Hence, after the series has vanished for a time from consciousness, and the initial member is in some way reproduced, α recalls β with an energy greater than that with which it recalls γ, and so on; thus the series is reproduced in its original order.[1]

Thus in Herbart's psychology memory is no innate faculty, but is involved in the striving of presentations for self-preservation. Forgetting is the arrest of weak presentations by stronger presentations, and occurs because the simplicity of the soul makes it impossible for contrary presentations to coexist in consciousness. The obscured presentation has a latent power analo-

[1] Cf. Stout on the evolution and involution of series, op. cit., No. 51, pp. 334, 335.

gous to latent natural forces. Reproduction is the natural result of the irrepressible striving of presentations, and occurs *ipso facto* when obstacles are removed.

Beneke agrees with Herbart in combating the idea that memory is a faculty ; and, with some change of terminology, his theory of memory is, in general, not essentially different from that just given, except that, according to him, presentations are not really in conflict, but a presentation becomes obscured because certain psychic elements are transferred from it to others, and it returns to consciousness because it receives an increased quantity of those elements.[1] In his *Erziehungslehre* he considers the pedagogical aspects of the subject.[2]

Among disciples of Herbart may be mentioned Drobisch, Volkmann, Dörpfeld, and Steinthal.[3] Of these, Dörpfeld emphasizes the comprehensiveness of memory, showing that all acts of thinking, comparing, and judging, and even the simplest perceptions of daily life involve memory ; and as all reproduction is either immediate or mediate, as stated above, he attempts to reduce the laws of association to one law, and considers the important relation in which memory stands to thought. Steinthal considers the general principles of memory, and develops the Herbartian doctrine in harmony with his own theory of apperception.

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[1] See Lehrbuch d. Psych., Ch. III ; also Die Neue Psych. and Psych. Skizzen.

[2] Bd. I, §§20–22.

[3] Cf. Drobisch's Empir. Psych., §§31–41 and 117 ; also his Mathemat. Psych., passim ; Volkmann, Lehrbuch d. Psych., Bd. I, IV (this is especially valuable for many notes and references) ; Dörpfeld's Denken und Gedächtnis ; Steinthal's Psych. und Sprachwissenschaft, Th. I, §§79–91 and passim. Also, for a good account of the views of Dörpfeld and Steinthal see Fauth's Das Gedächtnis : Gütersloh, 1888.

VI.—*Lotze.*

Lotze, among recent philosophers, is the most able champion of the doctrine that memory is a "power of the soul to preserve impressions independently of physical conditions, and to unite them according to laws that have nothing in common necessarily with the modes of action of nerve-forces." Even if the opposite theory of the dependence of memory upon physical processes be tenable, he would choose the former for ethical reasons; for in the next sentence to that quoted he says: "Where two hypotheses are equally possible, one harmonizing with moral needs, the other at variance with them, nothing can turn the choice in favor of the latter."[1] Yet Lotze does not deny the influence of cerebral changes on the train of thought. On the contrary, he maintains that there is an interaction between mind and body, that "not only the nervous stimulation conditions a definite presentation, but also the presentation, emerging again in the course of remembrance, reacts and strives to reproduce that nervous state by which it was itself aroused in sense-perception."[2] But the brain is not the organ of memory; for, while admitting the indirect dependence of the train of thought on bodily processes, he says: "The doctrine of a special organ of memory, even as a mere means of support to the soul's own power of remembrance, is exposed to greater difficulties than is commonly thought. The objection that the cerebral mass, which is not unalterable, but undergoes slow renovation, could not, without confusion, retain for future use the impressed copies of countless impressions, is

[1] Medicin. Psych. §36, p. 473.

[2] Op. cit., p. 474.

met plausibly but not convincingly by reference to the countless undulatory movements of sound and of colored light that can simultaneously traverse the same atmospheric space without mutual disturbance."[1]

The images of sense-perceptions, Lotze argues, are not strictly images. The mind takes cognizance only of qualitative differences of stimulation. Sense-impressions are but *intensive* data by the help of which the mind by its own activity reproduces the external world. To suppose that the brain retains impressions of the countless images of sense is possible only on the hypothesis that each cerebral atom is capable of retaining in itself without mutual disturbance an infinite number of impressions. "Such a theory would simply contain many repetitions of the same supposition that we make once. If every several atom of the cerebral mass is capable of retaining without confusion numberless impressions, why should the soul alone, like the atom a simple being, be incapable of doing so? Why should it alone not possess the faculty of memory and recollection in itself without the aid of a corporeal organ, when we have to concede that faculty directly and without the mediation of a new instrument to every part of the assumed organ? Nay, we must, in fact, make the contrary assertion, that the retention and reproduction of impressions is possible, not to a number of co-operant cerebral particles, but exclusively to the soul's undivided unity. . . . . to admit an organ of memory would only lead to our having to attribute a memory to the soul, and also to regard the several atoms of the brain as souls whose power of remembrance assists ours."[2]

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[1] Microcosmus, Book III, Ch. III, §5.

[2] Loc. cit.

Lotze, however, does not neglect empirical facts, but considers the relation of sense-perception to reproduction, and discusses the morbid phenomena of amnesia and hypermnesia, finding nothing that contradicts his theory of memory. His discussion of our subject is one of the ablest arguments in modern times for the transcendental theory of memory. Yet, as Horwicz says: "From this man we can learn to think upon psychological questions in terms of physiology."[1]

VII.—*Fechner.*

Fechner, in accordance with his Spinozistic principle of the double-aspect nature of the world, writes thus of memory: "Remembrances (*Erinnerungen*) are developed from perceptions, under the supposition of a universal consciousness in which both are included. Without knowing the psycho-physic processes which underlie the one or the other, we can, nevertheless, conclude according to the functioning principle, that the psycho-physic conditions of remembrances are developed out of those of perceptions, under the supposition of universal psycho-physic conditions which the nature of the universal consciousness demands."[2] These theoretic views we need not dwell upon here. Of special interest, however, are his observations in regard to the relation between after-images and memory-images.[3] According to him, the chief differ-

[1] Psych. Analysen, Bd. I, p. 272. For Lotze's discussion of the train of ideas and criticism of those who explain memory after the analogy of physical inertia (Fries et al.), and of the Herbartian theory, see Microcos., Book II, Ch. III, and Outlines of Psych., Ch. II. Cf. also Metaphysic, B. III, Ch. II and V.

[2] Elemente d. Psycho-physik, II, p. 380. I translate *Erinnerung* "remembrance" rather than "recollection" because the latter implies volition, while the German word as used here and below is not limited to voluntary recollection.

[3] Op. cit. II, p. 468 seq.

ence between the two consists in a feeling of *receptivity* connected with the former, in contrast with a greater or less feeling of *spontaneity* connected with the latter. He observed carefully the difference between memory-images and after-images in his own experience, and questioning others skilled in introspection—such men as Volkmann and Drobisch—found great personal differences in the vividness and distinctness of the memory-images.[1] At one end of the scale stood Fechner himself, with memory-images "incomparably less distinct" than after-images; at the other end the painter, reported by Boismont, who, in a half-hour's sitting, obtained such a vivid memory-image of his subject that it served him afterwards as a model.[2] The one distinction between memory-images and after-images found by all observers was, that while the former can be modified at will, the latter cannot be voluntarily changed.

Fechner distinguishes five classes of phenomena in respect to persisting images: (1) after-images, *i. e.*, after-sensations; (2) memory-images; (3) memory-after-images; (4) phenomena of sense-memory; (5) involuntary hallucinations. By the third class—memory-after-images—Fechner means memory-pictures formed immediately after the perception, and having, there-

[1] Cf. also Galton's observations in regard to Mental Imagery, Enquiry into Human Faculty, p. 83 seq.

[2] The painter described his method of work after such a sitting in these words: "Je prennais l'homme dans mon esprit, je le mettais sur la chaise, où je l'apercevais aussi distinctement que s'il y eût été en réalité; et je puis même ajouter avec des formes et des couleurs plus arrêtées et plus vives. Je regardais de temps à autre la figure imaginaire, et je me mettais à peindre; je suspendais mon travail pour examiner la pose, absolument comme si l'original eût été devant moi; toutes les fois que je jetais les yeux sur la chaise, je voyais l'homme." Brierre de Boismont, Des halluc., p. 39, quoted by Fechner.

fore, a vividness comparable to that of the after-sensation. Under the fourth class he includes after-images that appear involuntarily some time after the original sensation.[1] These different phenomena form a sort of gradation between the sensory after-image on the one hand and the memory-image on the other. Fechner concludes from his observations that the processes underlying the phenomena of after-images and of memory-images are essentially the same. If the psycho-physic process is aroused within and passes outward, we have a memory-image; if it proceeds from without inward, we have an after-image.

VIII.—*Horwicz.*

Horwicz makes an important contribution to our subject, by showing the relation of memory to feeling.[2] He distinguishes remembrance (*Erinnerung*) from mere memory (*Gedächtniss*), by limiting the former to cases where the time and space distinctions of the original presentation are revived, and using the latter to denote cases where only the content of the presentation is renewed. Considering the subject from a physiological standpoint, he shows that acts of reproduction are so involved in all psychic life, that the assumption of a special organ for memory is inconceivable. He would rather ascribe the power of reproduction in varying degree to the whole nervous system. We find the germs of memory even in simple reflex action. In case of the decapitated animal or the sleeping man, a futile movement for removing a stimulus is followed by a more appropriate one. This would be impossible

[1] This phenomenon was first mentioned by Henle.

[2] See his Psych. Analysen, Bd. I, pp. 265–331; also pp. 160 seq., 256 seq., 369 seq.

without remembrance of the first movement and its ineffectiveness. In Pflüger's experiment, the segments of an eel's tail react *from* the flame, not into it. "Hence it follows that remembrance (*Erinnerung*) dwells in each central organ, yes, even in each smaller complex of nerve elements."[1] Yet we may regard the great commissures of the brain as the most important organs of reproduction. The manifold trains of commissural fibers seem specially adapted to function the linking of our ideas; and there is no other use to which they do seem adapted.

The assumption of this physical basis for remembrance does not, in the opinion of Horwicz, prove materialism; for many facts tend to show that these fibers were developed by psychic activity. Nor does the anatomical structure of the commissural system lend support to the Herbartian psychology; for there is no one point (corresponding to Herbart's statical point toward which all presentations strive) toward which all nervous paths converge.

If reproduction is a function of one class of the commissural fibers, *i. e.* those of the brain, then "reproduction is a special case of association." Taking the spinal cord as the simplest example of a central organ, its three classes of commissural fibers, the lateral (uniting sensory and motor cells and functioning simple reflex actions), anterior (connecting motor cells with each other and associating movements), and posterior (connecting sensory cells with each other and associating sense-impressions), afford a physiological explanation of the association of presentations.

This, however, does not explain the preservation of

[1] Op. cit., p. 278.

impressions in memory. The simple remembrance of a former stimulus seems to be confined to the same nerve-fiber and cell as the original impression. But this is not so. We fail to recognize at once known objects in unfamiliar places. The prick of a pin we recognize whether on the foot or on the hand. Therefore remembrance does not occur in the simplest case without association.

Moreover, every sensory nerve-path passes through several cells. Hence it is probable, Horwicz concludes, that the sensation and the remembered sensation do not have their seat in the same cells, but that some cells are devoted exclusively to memory. Probably in the higher central organs a memory-fiber, at every station of the afferent nerve-path, branches off and ends in a terminal cell. In these memory-cells we must assume that a trace of the sense-impression is deposited, which trace is associated with new impressions as soon as the stimulus strikes the neural path concerned. We must not, however, suppose that this storing-up process is something peculiar to the so-called memory-cells. From the identity of nerve-structure we must rather assume that the process is the same on the whole neural path. But in the conducting fibers and the intermediate cells, we may suppose that, on account of their frequent stimulation and other functions, the storing-up process cannot so easily occur, but only a disposition to definite associations remains. "The universal law of the association of nervous excitations appears at the first stage as an accessory sensation (*Mitempfindung*) and an accessory movement (*Mitbewegung*), in the course of the nerve-path as a disposition, at the end of it as an after-sensation. The after-sensation is related to remembrance

as the accessory sensation and accessory movement to the association of sensations and movements."[1]

Thus reproduction is a kind of association, and association in its widest sense, *i. e.* the transmission of an excitation from one nerve-fiber to others, is a universal function of the central organ. Anatomical necessity conditions sensory and motor association originally, since the excitation must spread equally over all cells connected with the stimulated center. In this primary stage association appears as accessory movement or accessory sensation. Gradually, by chance or otherwise, the spread of the stimulus preponderates in one direction. This preponderance gradually increases until, finally, a decided inclination for a definite direction is formed. This transmission of a stimulus with a decided disposition is association in the narrowest or usual sense.

Leaving the motor side, and attending exclusively to the association of sensations, Horwicz gives the following definition as a résumé of what has been said: "*Reproduktion ist eine Uebertragung von Reizen überwiegend nach der sensiblen Seite in bestimmten durch Dispositionen angegebenen Richtungen und auf Residuen, die von älteren Reizzuständen aufbewahrt sind.*"[2]

Horwicz discusses the theory that explains memory after the analogy of the persistence of physical forces, and shows that the law of inertia cannot hold in the mental world in the same way as in the physical. In order really to understand the persistence that occurs in memory, we must understand the nature of sensa-

[1] Op. cit., p. 289. It should be stated that Horwicz offers this explanation only as an hypothesis.

[2] Op. cit., p. 291.

tion. The latter, however, is unknown. Therefore we can at most only investigate the relation of remembrance to sensation. It is apparently so easy to distinguish between sensation and remembrance that most psychologists have not thought it worth while to notice the distinguishing marks of the two. Fechner, however, gave special attention to this;[1] and the phenomena discussed by him show that remembrance is distinguished from sensation by the greater influence of the will in the former.

The persistence of presentations is no mere mechanical one. If the volition is the essential mark that distinguishes the remembrance from sensation, then it is also the ground of the persistence of the presentation. The persisting presentation is itself volition, activity, impulse. A sensation is essentially an impulse to movement, consequently the remembrance must be such an impulse stored away in the nerve-element. We may conceive of this physiologically as if a spring were made tense by pressure. Then suppose that a second spring is so united with it that when the first is pressed the second is made tense also, but is not relaxed when the first is relaxed. Imagine an infinite number of such spring-systems, of which now these, now others are affected, and in the elasticity of the second springs we have a fitting illustration of the preservation of the sense stimuli in memory.

Horwicz considers the relation between the conscious and the unconscious in our mental life, and shows that the transition from the one state to the other is ordinarily a gradual one. All psychic activities divide into the two classes of conscious and unconscious processes. These two affect each other

[1] Cf. supra.

reciprocally, the conscious drawing forth the unconscious, and the unconscious pressing upon the conscious.

The original sensation, the perception of a nervous excitation, is an impulse that shows itself immediately by translation into motion. The sensation also remains such an impulse if, instead of the motor path, it strikes the memory-cells. We have proof of this in the fact that presentations as well as sensations can originate movements, and that even unconscious presentations are wont to have clearly motor effects (*e. g.* in the case of the instantaneous movements for retaining one's equilibrium called forth by the unconscious or weakly conscious idea of the circumstances of the situation). How does this impulse suddenly become latent, and again suddenly become active? Or, in other words, how does the presentation suddenly vanish, and again suddenly appear? We say that a presentation vanishes when the attention is turned to something else. But it is a poor excuse for letting the soup boil away, that the cook was sprinkling the roast. It is commonly supposed that the soul, on account of its unity, is capable of but one presentation at a time.[1] But the unity of the soul does not explain the facts of the unconscious. How can we conceive the latter in terms of physiology?

The physiological explanation is found in the mechanism of inhibition or arrest. The strength and life of the *feeling* connected with a presentation gives it the power to obscure others, just as it is the will (*i. e.* the result of feeling) that inhibits movements. The attention may be divided among indifferent presentations, but presentations highly colored

[1] Cf. Herbart, supra.

with emotion thrust back all that resists them. We can easily note the feeling of effort that it costs to bring foreign ideas or sensations back to consciousness.

On the other hand, the return of presentations to consciousness is not so simple a matter as the Herbartians suppose. According to them, the presentation flies up like the spring when the pressure is removed. But the incalculable kaleidoscopic interplay of our presentations does not seem to conform to any such simple rule of arithmetic. "Not simple inertia, mere continuance, is sufficient to explain the persistence of presentations, but a continuing impulse, a constant act of life. Not the mere entrance of a new (opposite or however otherwise related) presentation is sufficient to arrest the former, but there must proceed from the new presentation a voluntary act of inhibition. So also the mere removal of the inhibition is not sufficient to allow the arrested presentation to appear again in consciousness, but living, and probably voluntary, relations must prevail between the presentations that arouse each other. Living impulses must condition the coming and going of presentations."[1]

In the development of our knowledge of the external world, movement and sensations of movement play a most important role. "But movement is the immediate result of the impulse involved in the sensation, *i. e.* of feeling. Consequently, we have to look upon feeling as the proper basis of remembrance." As feeling conditions voluntary and involuntary attention, thus, according to Horwicz, feeling is the ground of the persistence of presentations and of voluntary and involuntary remembrance. But the ground

[1] Op. cit., p. 314.

of the persistence of a presentation is a continuing motor impulse, *i. e.* the impulse to respond to a definite stimulus with a definite movement.

If feeling is the vehicle of the train of thought, it may seem strange that it is so hard to remember feelings. But it is not feeling, as such, which is the primary element in remembrance, but feeling in its necessary union with the feeling of motion. We can remember a feeling to the extent that we can reproduce the movements involved in it. Feeling is not directly, but indirectly, the basis of association. "Hence is explained how, on the one hand, feelings can most powerfully control and condition the train of our presentations, and how likewise, on the other hand, it is so difficult, yes impossible, to reproduce a feeling in itself."

Horwicz considers also the laws of the association of ideas, and maintains that in the traditional laws handed down from Aristotle, contrast is omitted, but that it is easily explained by his theory of feeling. Further, he considers the relation of his theory to the unity of consciousness, and finds the possibility of that unity in the connection of the nerve-cells in every direction.

IX.—*Wundt.*

Wundt's[1] theory of memory may be briefly stated. According to him, memory in its widest sense as the general power to renew presentations is the prerequisite for imagination and intellect. It has both a physical and a psychic side. On the physical side, it has its ground in those changes of sensibility that facilitate the return of conditions of stimulation that have once been

[1] See his Grundzüge d. Phys. Psych., Bd. II, Ch. 17, 2te Aufl.

aroused. The reproduction of vanished presentations depends on persisting dispositions to these presentations. All the phenomena of habit and of adaptation to circumstances indicate that the residua of impressions are functional dispositions. But "all reproduction proceeds from the presentations which are in consciousness, and the presence of unconscious dispositions does not revive the presentations, unless the necessary conditions for the forming of associations are present in consciousness itself. . . . The dispositions unconsciously present, and the degree of their registration, determine only what presentations in general can enter consciousness, but the actual entrance of any given presentation is always occasioned by the condition of consciousness itself."[1] Thus on the psychic side, reproduction is conditioned by psychic stimulation; and the starting point of recollection is always the present content of consciousness.

X.—*Organic Memory.*

The modern theory of unconscious or organic memory began long before Hering's famous lecture before the Imperial Academy of Sciences, at Vienna, in 1870. The germs of the doctrine are found in Malebranche. This Cartesian, as we have seen, explained memory by supposing that, when the brain-fibers have once received an impression, they acquire a facility of receiving the same impression again, just as the branches of a tree that have been bent in a certain manner acquire a readiness to be bent afresh in the same manner. Thus we think of the same things when the same cerebral processes recur. Moreover, in Malebranche's opinion, not the brain alone acquires

[1] Op. cit., p. 319.

this disposition to repeat changes that have once occurred, but the same is true of all parts of the body. Thus he says that, apart from consciousness, there would be no difference between memory and the other habits of the body.[1]

That Malebranche uses the word "habit" in referring to the repetition of organic processes, instead of the phrase "organic" or "unconscious memory," does credit to his clear thinking and to his care in the use of language. The defects in Malebranche's psychology of memory were due to the crude condition of physiological science at his time; but the distinction that he made between *memory* and *habit*, making the latter the genus and the former the species, has been, I think, the prevailing one among physiologists ever since.

Moreover, among German writers, Jessen, in his *Versuch einer wissenschaftlichen Begründung der Psychologie*, published in 1855, expressed a somewhat obscure doctrine of organic memory.[2] In his opinion, memory could, in a certain sense, be attributed even to things without life; for solid bodies retain impressions made upon them.[3] In illustration of this he cited such facts as the following. Musical instruments that have always been correctly played are superior in purity of tone. It is impossible to get an entirely pure note from a flute that has been wrongly played. The particles of the wood reproduce the accustomed vibrations. In all living organisms thought appears as a force existing in matter. Why, he asks, should not

[1] Cf. my preceding article, footnote 2, p. 68.

[2] Cf. Huber, Das Gedächtniss, p. 24; and Fauth, Das Gedächtniss, Ch. II.

[3] Cf. Draper, Conflict between Science and Religion, p. 132 seq.

the phenomena of thought be derived from thinking matter as well as the phenomena of electricity from electrical substances?

According to him, thoughts arise in one part of the brain, and come to consciousness in another part. The higher mental activities are located in the cortex of the cerebrum. If a clear image of an absent person is perceived, an efferent process from the brain-ganglia to the retina of the eye must be reproduced. Memory is no abstract property of the mind, but a universal property of nerves. In some inexplicable way, every impression persists in the nerves. The oftener and more strongly impressions are repeated, the more permanent becomes the effect upon the nerves, and the more easily can the appropriate movements recur. The ideas persist, not in the nerve-fibers, but in the nerve-cells. Hence, the special seat of memory is in the gray columns of the spinal cord, in the gray matter of the cerebral ganglia, and in the cortex of the brain.

This theory of organic memory was first brought into prominence, however, by Hering's lecture "On Memory as a Universal Function of Organized Matter,"[1] delivered before the Imperial Academy of Sciences, at Vienna, May 30, 1870. Starting from the law of the functional interdependence of matter and consciousness, and recognizing the necessity of supplementing physiology by psychology, he attempts to regard, under a single aspect, a great number of phenomena belonging partly to the conscious, partly to the unconscious life of organized beings. This common attribute is the

[1] Ueber das Gedächtniss als eine algemeine Function der organischen Materie. I have been unable to obtain this in the German, so have been obliged to refer to Butler's translation. See Butler's "Unconscious Memory," Ch. VI.

memory or power of reproduction in organized matter. Examining memory closely, we find it to be a power of our unconscious as well as of our conscious life. Ideas appear for a moment on the stage of conscious life, then vanish, and reappear to-morrow. Where have they been meanwhile? They do not exist as ideas. Between the "me" of yesterday and the "me" of to-day are vast abysses of unconsciousness. What is continuous is the disposition of nerve substance, in virtue of which it repeats to-day the processes of yesterday. Again, the train of our ideas seems often to disregard the order that would obtain in a series of cerebral processes. Here the organic processes are connected in an orderly manner, but not all the links of the series appear in consciousness. The bond of union, then, must be in the unconscious world; and as, "for purely experimental purposes, 'matter' and the 'unconscious' must be one and the same thing, so the physiologist has a full right to denote memory as, in the wider sense of the word, a function of brain substance whose results, it is true, fall, as regards one part of them, into the domain of consciousness, while another, and not less essential, part escapes unperceived as purely material processes."[1] Moreover, all the phenomena of habit and our automatic processes give repeated illustration of the power of reproducing organic processes without consciousness, due to the memory of the nervous system.

The same power of reproduction is found in other kinds of organized matter. The more a muscle is used the stronger it becomes. We have here, in its simplest form, the same power of reproduction that, in a more complicated form, is found in nervous substance. The

[1] Butler's translation, p. 111.

same is true in greater or less degree of all our organs. Increased use, alternating with periods of repose, gives the organ increased power of execution, increased power of assimilation, and a gain in size. Three things are involved in this: (1) internal alteration of the molecular disposition of the cells; (2) increase in the size of the individual cells; (3) multiplication of the number of cells. The last is a result of the other two; for, when cells have attained a certain size, they give rise to others that inherit more or less completely the qualities of the original cells.

This same power of memory is seen, according to Hering, in the facts of heredity. Acquisitions made during the life of the individual organism are transmitted to its offspring. In some mysterious way, the nervous system, in spite of its thousandfold subdivision into cells and fibers, forms a united whole, and even each cell of the more important organs is intimately related with the whole. This seems to be especially true of those germs marked out for independent existence, and each of these germs bears its part in the activities of the whole organism. Hence the offspring inherits the acquired peculiarities of the parent, or rather remembers them; for the descent of these peculiarities is a reproduction by organized matter of processes in which it took part as a germ in the parent organism, and of which it seems still to retain a remembrance, since, on occasion of given stimuli, it responds to them as the parent organism responded to the same stimuli. Thus the facts of heredity are as wonderful as when a gray-haired man remembers the events of childhood, but no more so.

If the germ can reproduce characteristics acquired by the parent, all the more will it be able to reproduce

those that have developed through countless generations in the organized matter of which the germ is a fragment.

"An organized being, therefore, stands before us a product of the unconscious memory of organized matter. . . . . Thus regarded, the development of one of the more highly organized animals represents a continuous series of organized recollections, concerning the past development of the great chain of living forms, the last link of which stands before us in the particular animal we may be considering. As a complicated perception may arise by means of a rapid and superficial reproduction of long and laboriously practised brain processes, so a germ, in the course of its development, hurries through a series of phases, hinting at them only."[1]

The memory of organized substance is especially seen in the phenomena of instinct. This power of remembering the experience of ancestors must be ascribed to the new-born infant as well as to animals. The brain processes upon which consciousness depends have had a less ancient origin than those relating to physical needs. Hunger and the reproductive instinct affected the oldest and simplest forms of organic life. Hence the memory of organized matter is strongest in respect to them.

Thus Hering finds the explanation of habit, instinct, and heredity in a phenomenon of consciousness, and, according to him, memory is a biological fact. "Man's conscious memory comes to an end at death, but the unconscious memory of Nature is true and ineradicable."

An interesting application of this theory to disease

[1] Butler's translation, p. 125.

has been made by Dr. Creighton.[1] Basing his opinion upon the principle of Hering that memory is a function of all organized matter, he uses the word *memory* in no figurative sense when he says: "Embryonic development, growth, and the continuity of organic life are the actual and explicit manifestations of that memory which was potential, implicit (and known to exist by inference only) in the sperm-particles and egg; just as consciousness is the actual and explicit manifestation of that memory which is potential, implicit (and known to exist inferentially) in the vast reserve of the unconscious which is at any given moment behind the scenes."[2] Applying his principle to many chronic diseases, he finds them characterized by a reversion to older and lower modes of life. The catarrhs, for example, "are a return, for a short time, to a more elementary, primitive, or embryonic kind of epithelial function."[3] Hence the value of alteratives consists in their habit-breaking or memory-breaking action. An alterative "effaces the memory of morbid action by substituting an action like it."

The terminology of those writers who use the phrase "organic memory" has justly been criticized. The phrase is at best a figure of speech, a kind of metonomy or synecdoche. All memory involves retention, and reproduction as well. But not all retention and reproduction involve memory. In the poverty of scientific language, the word *memory* is a very convenient term, and the extension of its meaning to include all retention of impressions by organic matter, and the functional disposition to repeat organic processes

[1] See his Unconscious Memory in Disease.

[2] P. 16.

[3] P. 37.

in general, has the advantage of showing the relation of memory proper to the physical processes that form in large part the subject-matter of physiology and biology, and it helps to unify in one concept many diverse phenomena—an economic device that is often valuable. But there are serious objections to such a use of the word *memory*. The idea of consciousness is so deeply involved in the connotation of the word that it should be restricted to its ordinary, limited use. And the extended use of the word is unnecessary; for what is this functional disposition to repeat organic processes but the law of habit? Habit and memory are intimately related, but the word *habit* is the broader term. To say that a diseased organ has lapsed into a bad habit is more in accord with custom than to say that it remembers an old and lower mode of functioning. To speak of the *habit* of a cell is not as confusing as to speak of *cell-memory*. The fact of the retention of impressions and the reproduction of physiological processes by organic matter, no one will deny; but "it is no more fitly called 'organic memory' than are the molecular alterations produced in the wood of an old Cremona."[1]

XI.—*Conclusion.*

The number of modern writers who have discussed the subject of memory is legion. It is impossible to mention them all here. But the writers whose views have been presented in this article are representative men, and their theories illustrate the conceptions of memory prevalent in their respective schools. The continued Platonic and Aristotelian influences may still be noticed in these modern theories; the former

[1] Prof. Ladd in Elements of Phys. Psych., p. 554.

appearing especially in the transcendental conception of memory, which was taught by the German idealists, and appeared in modified form in the Scottish School, and later found its ablest champion in Lotze; the latter appearing more or less in the empirical conceptions of the Associationists, Herbartian as well as English, and in modern physical theories.

Finally, it must be plain that, whatever be the relative merits of the idealistic and the physiological theories of memory, the facts of introspection have been pretty thoroughly worked over in the continued discussions of memory, from the days of Plato and Aristotle down to the last German student who has contributed a thesis *Zur Theorie der Reproduction*. After our historical orientation, the quarter of the horizon that looks most promising is in the direction of empirical study. In a future article I hope to notice briefly some recent views omitted in the present chapter, together with the empirical studies of Ribot, Kraepelin, Ebbinghaus and others, and to add the results of some experiments of my own.

PERSONAL EQUATION.[1]

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II.

VARIATIONS IN THE AMOUNT OF PERSONAL EQUATION.

A certain variability in the personal equation is generally admitted. Those that assert its constancy, like Wolf,[2] separate it into two factors, one depending on the circumstances of observation, the other on the observer, and mean that the last is practically constant. But even this must not be taken too strictly. There are changes of personal equation while the circumstances of observation so far as known remain the same—progressive for some observers; quite without order for others. Both are illustrated in the equations of Main, Rogerson and Henry at Greenwich from 1841 to 1853 (observations by eye and ear). From 1846

[1] Continued from AMERICAN JOURNAL OF PSYCHOLOGY, November, 1888. The last section of the first part dealt with the absolute personal equation, and the last paragraph but one with a method of getting the absolute personal equation from transits of real stars. The following wholly different method, applied by Bredichin (*Annales de l'Observatoire de Moscou*, Vol. II, part 2, page 69), may be mentioned here. He replaced the central wire by a plate of metal having its sides parallel to the other wires. He observed at the wires in the usual way and by immergences and emergences at the edges of the plate. The mean of the last two gives approximately the true time of the transit of the middle of the plate (within 0.02 s for Bredichin), from which the personal equation for the wires can be obtained.

[2] Wolf: *Annales de l'Obs. de Paris, Mémoires* VIII, 153. Hough: American Assoc. Proceedings, XVI, 22. Wolf admits changes through fatigue, etc.

the calculations are given by the Astronomer Royal,[1] the earlier ones are supplied by Peters.[2]

		s.		s.
1840	M—R =	−0.15	M—H =	..
1841	=	+0.08	=	−0.09
1842	=	..	=	−0.01
1843	=	+0.20	=	−0.02
1844	=	+0.18	=	−0.05
1845	=	+0.20	=	−0.12
1846	=	+0.26	=	−0.05
1847	=	+0.35	=	−0.03
1848	=	+0.37	=	−0.04
1849	=	+0.39	=	−0.05
1850	=	+0.45	=	−0.11
1851	=	+0.47	=	−0.11
1852	=	+0.63	=	−0.00
1853	=	+0.70	=	+0.03

The increase of M—R is said by Dunkin, a fellow observer, to be due to a change in Rogerson's method of observing, but he fails to specify the nature of the change.

Further evidence of variability is furnished by changes in the difference between the personal equation for eye and ear and chronographic observations. Hilfiker found differences in his own personal equation by the two methods as follows:[3] (1883) 0.074 s, (1884) 0.086 s, (1885) 0.053 s, (1887) 0.022 s. Those below are given for the Greenwich observers:[4]

Excess of Clock Correction by the Eye and Ear Method.

	HT. s.	AD. s.	T. s.	L. s.	H. s.
1885	−0.02	+0.06	+0.04	+0.10	+0.63
1886	+0.05	+0.08	+0.05	+0.12	+0.59
1887	+0.03	+0.05	+0.02	+0.12	+0.58

[1] Greenwich Observations.

[2] *Astronomische Nachrichten*, XLIX, 1.

[3] *Astronomische Nachrichten*, No. 2815.

[4] *Observatory*, Jan. 1888, p. 95.

The personal equation varies even from day to day. To mention only two from many instances, I may refer to the comparisons before mentioned, of Nehus and Wolfers, and those of Hirsch and Plantamour.

The personal equation for the sun and moon is different from that for the stars; and that for the first limb of the sun and moon is different from that for the second.—The observation of star transits by both methods has been described. In observing transits of the sun and moon, it is the custom not to bisect the disk, but to observe the transits of the limbs, and from them to get the time for the center. Astronomers have generally had a different feeling in observing stars and limbs; it is one thing to estimate the distance of the edge of a large bright disk from the wire or to fix the contact, and quite another to do the same thing for a star. It is also more difficult to observe the advancing edge when the wires are faint from the glare, than the retreating edge when the wires are black lines on a bright surface.

The feeling of the astronomers is justified by the probable errors. Dr. Robinson,[1] observing at Armagh in 1830 by the eye and ear method, found for a transit of the sun's first limb a probable error of ± 0.116 s, for the second ± 0.087 s, for stars ± 0.097 s. Downing found[2] from a hundred complete transits of the sun at Greenwich in 1874 and 1875, as follows:

	I Limb.	II Limb.	Star.
	s.	s.	s.
Prob. error of transit of one wire,	± 0.072	± 0.063	± 0.051
Prob. error of complete transit,	± 0.024	± 0.021	± 0.017

[1] Proceedings of the Royal Irish Academy, VII, 371.
[2] *Monthly Notices*, XXXVIII, 102.

Other errors are here involved, but not such as to affect the relative amounts.

Besides the difference in certainty, the presence of a real difference of personal equation for the two limbs was early recognized. In 1848 a large discordance was found in the tabular errors of the moon in right ascension as deduced from the altazimuth observations of Dunkin and those of H. Breen. From a year's observations of the first limb the difference amounted to 0.46 s, from those of the second to 0.30 s. In 1869 Dunkin investigated the subject on a basis of six years' observations of the moon with the altazimuth and transit circle, and in 1874, on a basis of ten years' transit observations of the sun.[1] Five years later Neison studied the transit observations of the moon from 1863 to 1876.[2] The results are all in substantial agreement, as shown by the following tables:

Observations on the Moon.

DUNKIN, 1869:

	Transit circle.		Altazimuth.	
	I Limb.	II Limb.	I Limb.	II Limb.
C—D	+0.034	+0.032	−0.047	−0.028
C—E	+0.112	+0.077	+0.104	+0.048
C—JC	+0.132	+0.038	+0.207	+0.004

NEISON, 1879:

RELATIVE PERSONAL EQUATIONS.[3]

	I Limb.	Wt.	II Limb.	Wt.
C—D	=+0.012	4	+0.032	3
C—E	=+0.106	9	+0.065	7
C—JC	=+0.125	8	+0.038	6
C—L	=−0.034	4	−0.058	3
C—AD	=−0.007	2	−0.109	2
C—T	=−0.040	1	−0.093	½

[1] *Monthly Notices*, XXIX, 259; XXXV, 91.

[2] *Monthly Notices*, XL, 75.

[3] The signs in this table are changed, to make comparison with the rest more easy.

ABSOLUTE PERSONAL EQUATIONS.

	I Limb. s.	II Limb. s.
D	$+0.025 \pm 0.012$	$+0.004 \pm 0.014$
E	-0.060 ± 0.009	-0.041 ± 0.010
C	$+0.045 \pm 0.008$	$+0.026 \pm 0.009$
JC	-0.074 ± 0.010	-0.014 ± 0.012
L	$+0.075 \pm 0.015$	$+0.066 \pm 0.020$
AD	$+0.006 \pm 0.022$	$+0.132 \pm 0.026$
T	$+0.063 \pm 0.025$	$+0.183 \pm 0.050$

The numbers in the first table are really mutual differences, obtained from averages of the tabular errors of the moon as found by the different observers, and contain, especially those for the altazimuth, other errors than those of personal equation. Those in the second table are averages of the mean personal equations of the fourteen years, weighted for number and distribution of observations. Those of the third are calculated on the assumption that the personal equations compensate each other and that their sum equals zero. The results for A, D, and T are less certain, because of the small number of their observations.

Observations on the Sun.[1]

DUNKIN, 1874:

	I Limb.	II Limb.
C—D	+0.042	+0.002
C—E	+0.103	+0.019
C—JC	+0.150	−0.001
C—L	+0.016	−0.037
C—HC	+0.034	−0.126

The differences come out most strongly for most observers in transits of the first limb where the glare is most troublesome, but not for all. Dunkin suggests no explanation but a difference in the "methods of estimating by the eye the positions of the two limbs in

[1] The last two personal equations are not in Dunkin's table, but are figured from other tables given.

transit as affected by irradiation." Dr. Robinson also assigned irradiation as the cause, and by lessening it was able to lessen the error materially.

The personal equation changes with the magnitude of the star.—An early notice of such a connection, perhaps the earliest, comes from the elder Struve.[2] About 1869, the attention of Argelander was called to a difference of 0.66 s in the right ascension of the minor planet Egeria as determined at Bonn and Leyden. An examination of the observations, and exclusion of other sources of error, reduced the possible causes to a different perception of the instant of transit for faint and bright stars. He had before suspected that he himself observed the transits of very faint stars differently from those of bright ones. To test his suspicion, he examined his observations of variable stars as far as the ninth magnitude, but without finding any appreciable difference. With still fainter stars, 9.2 and 9.3 magnitudes, he found a slight tendency to observe too early, but not more for the latter than 0.15 s. Since this was insufficient to account for the difference in the position of Egeria, he suggested a variation of the same kind in the observatory of Leyden, where the observer might have been less experienced.

It was, indeed, at that observatory, though several years later, that the question was first thoroughly investigated.[3] In connection with a study of the orbit

[1] Introduction to Armagh Catalogue.

[2] *Positiones Mediae*, lxiv.

[3] In the same year as Argelander's demonstration of the difference, the question was touched experimentally by Rogers (*American Journal of Science and Arts*, Second series, Vol. XLVII, p. 297). His experiments dealt with several other causes of variation than the size of the star, and though, in comparison with later experiments, his are crude and, as the author himself recognizes, not

of Mars by Gill, in 1877, the place of a number of stars was fixed at the same time by twelve important observatories in different parts of the world. The resulting right ascensions showed differences that Gill traced, in part at least, to variations of personal equation with magnitude. Following such a suggestion, previously made to him by Gill, H. G. v. d. Sande Bakhuyzen, of the observatory of Leyden, reviewed the results. On the assumption that the mean of the right ascensions from the ten observatories (two were omitted for special reasons) was exact, he found the differences of each from the mean and plotted curves, to show the effect in each of a change of brightness in the star.[1] The form of these curves is distinct for the different observatories, and very interesting, but on a narrow numerical basis.

To put the question to a direct experimental test, Bakhuyzen and his assistants observed a number of transits, using for half of each the full objective, and,

definitive, they will be referred to several times in what follows—a word, therefore, as to the manner of them. The artificial stars were made of paper and mounted on wires attached to a chronograph drum. Their transits behind a fixed wire and the record of the observer were taken electrically on the same drum. This record was measured to tenths and estimated to hundredths of a second. In general, eight stars were used, and ten turns of the drum made a series. In his experiments on magnitude, he and his assistant observed the transits of five paper stars, the first of which was larger than the rest. The result, in the form of relative personal equations, was: For the large star, R—T = —0.233 s; for other stars, R—T = —0.152 s; difference, 0.081 s. To show the same result in another way: the positive excess of T's personal equation for the large star over the small ones was 0.047 s; B's negative excess was 0.036 s; the difference of R and T, 0.083 s. No such clear difference could be traced in the observation of real stars. The experiments evidently do not touch the question of brightness apart from apparent area of the disk.

[1] These curves accompany Gill's article, *Monthly Notices*, XXXIX (1879), 98, which is of later date than Bakhuyzen's communication in the *Astronomische Nachrichten*, XCV, 187. Gill touches the question also in the same volume of the *Monthly Notices*, p. 434.

for the other half, the objective reduced by a diaphragm to $\frac{1}{7.87}$. This reduced the stars about 2.3 magnitudes. To avoid errors, the diaphragm was used alternately for the first and second halves. These are the mean results for observations by the chronograph method :

REDUCED MINUS FULL OBJECTIVE.

	First half transit reduced.	No. of stars.	Second half transit reduced.	No. of stars.
E. F. Bakhuyzen,	+0.071	8	+0.070	8
Wilterdink,	+0.049	11	+0.047	8
Stieltjes,	+0.060	8	+0.034	9

Continuing his experiments, Bakhuyzen later published experiments with both methods of observing, in part of which the diaphragm was used, and in part a grating of fine copper wire that reduced the stars 2.8 magnitudes.[1] The following are the mean results :[2]

REDUCED MINUS FULL OBJECTIVE.

	s.	s.	Mean mag.	No. of obs.
	Chronograph Method.			
E. F. Bakhuyzen,	0.049	± 0.007	5.5	80
	0.045	± 0.013	3.1	14
Wilterdink,	0.039	± 0.009	5.3	48
	0.035	± 0.019	2.5	5
Stieltjes,	0.041	± 0.008	5.3	65
	0.045	± 0.012	3.1	15
	Eye and Ear Method.			
E. F. Bakhuyzen,	0.054	± 0.013	5.1	41
	0.065	± 0.023	3.0	8
Wilterdink,	0.023	± 0.020	5.4	16
	0.128	± 0.033	2.1	4
Stieltjes,	0.041	± 0.014	5.5	37
	0.103	± 0.037	3.5	3

These tables show, in brief, that all the observers at Leyden observed bright stars too early by both methods.

[1] *Vierteljahrssch. d. Astronom. Gesell.* XIV, 408.

[2] The mean magnitudes in the table are for full objective.

This increase in personal equation as magnitude diminishes probably has more than one cause. Wundt sees in it a close parallel of the increase in the length of the simple reaction time when the stimulus is diminished.[1] That is due to a change from the motor form of reacting, in which the attention is centered on executing the motion of recording, to the sensory form, where it is centered on catching the phenomenon to be recorded. Indeed, he suggests this kind of change as a general explanation of the change of relative personal equation. A change of attention might also affect observations with the eye and ear, as well as those with the chronograph.[2] In some cases this further cause also co-operates. It was explained in the first part of this paper that some observers, in using the chronograph method, anticipate the actual transit. They tap the key so as to make the sound or the muscular sensation of the closing coincide with the bisection.[3] If an observer does so, "he will be nearly certain," says Gill, "to press too soon for very bright, and too late for very faint stars, because the larger disk and the rings of a bright star will make it appear closer to the web than the small, sharp disk of a faint star, at the same angular distance from the web."

[1] *Physiologische Psychologie*, 3d ed., II, 273.

[2] Compare the observations of Hartmann, mentioned in the first part of this paper, p. 29.

[3] This way of observing, if not the best astronomically, is no doubt the natural one—a special case of the general tendency that makes man forethoughtful. All the persons tested by Mitchel (first part, p. 27) anticipated. That it is common among astronomers also, may be argued from the size of the personal equations. The shortest reaction-times found by psycho-physical experimenters for recording the reception of an instantaneous and expected stimulus (a sound or a certain position of a pointer on a dial) are about 0.075 s. It is clear, therefore, that observers whose records follow the actual bisection by less than about 0.075 s begin the motion of recording before the phenomenon has actually appeared. Personal equations by the chronograph method of less than 0.075 s are, I believe, by no means exceptional.

Yet another cause is suggested by Newcomb.[1] In comparing the right ascensions given by observations on the transit circle and transit, he found that those given by the former for faint stars were too great. After explaining that the spider lines appeared finer and the field less brightly illuminated in that instrument, he continues: "The effect of the latter cau will probably be to make an observer later in recording a transit. . . . The stray light which surrounds a bright star forms a bright ground for the dark transit wire a perceptible time before the star reaches the wire, and thus the approach of the two objects is distinctly seen. As we take fainter stars the stray light disappears and the approach is less distinctly seen. Thus the effect in question will be exaggerated as the star grows fainter."

The personal equation is influenced by the direction of the star's motion.—I have already mentioned, in summarizing Wolf's experiments (first part, page 36), that his personal equation was 0.04 s greater when the star moved from left to right than from right to left. In a single experiment, where he lay down at right angles to his telescope and thus observed with the star moving along the vertical meridian of his eye, his personal equation was 0.127 s, against 0.118 in the ordinary position. Kaiser found (first part, page 32) only one of the four observers at Leyden who was affected by change of horizontal direction, though all four were affected by change from a horizontal to a vertical direction. About the same time (1867), in a treatise presented to the Austrian Academy of Sciences, K. von Littrow discussed the differences of

[1] Observations at the United States Naval Observatory, XIV (1867), Appendix III, p. 27.

personal equation in the east and west positions of the "broken transit" or reflection theodolite.[1]

In this instrument, to facilitate observation and give greater steadiness, the rays of light gathered by its objective are bent at right angles by a prism at the level of its transit axis, where one of the trunnions is made to do duty for the ocular end of the tube. In an ordinary transit instrument, stars south of the zenith appear to move from right to left, between the zenith and the pole from left to right, and below the pole again from right to left. Zenith stars themselves may be observed with motion in either direction according to which side of the instrument the observer places himself. In the "broken transit," however, the apparent motions are vertical or oblique. Suppose such an instrument set so that the telescope tube moves in the plane of the meridian, and the trunnion eye-piece points east. The observer then faces west, and without changing his position can sweep the whole of his meridian from the northern to the southern horizon. He can do the same thing when the instrument is turned around and he faces east. But there is this difference: when the observer faces west, all stars south of the pole seem to move upward in his field of view; zenith stars directly upward, stars between the zenith and the pole upward from left to right, those south of the zenith upward from right to left, and stars below the pole obliquely downward from right to left. When the observer faces east the direction of motion is reversed: all stars south of the pole seem to move downward, those between the zenith and the pole obliquely from left to right, south of the zenith obliquely from right to left, and below the pole

[1] *Astronomische Nachrichten*, LXVIII, 369.

obliquely upward from right to left. Observations on such an instrument are, therefore, admirably suited to bring out any differences that depend on direction of motion.

Differences of clock corrections found in the two positions of the instrument were noticed in 1864 by Weiss; and in 1865, differences between the personal equations of Föster and Weiss in the two positions were also noticed. The latter then calculated from his earlier figures, making certain assumptions, the difference of personal equation in the two positions. The amounts are somewhat irregular, but the following are the means found:[1]

OBSERVER EAST MINUS OBSERVER WEST.

	Weiss.	Bruhns.
	s.	s.
Eye and ear,	−0.166	+0.072
Chronograph,	−0.214	−0.099

The observations on which these figures rest were of stars at about the same distance from the pole, and, therefore, cannot show the changes that very likely enter when that distance is changed, and with it the rate and obliquity of motion.

Littrow found further proof of the reality of the difference introduced by the direction of motion, in the tests made at Greenwich in 1852 and 1853 with the "binocular eye-piece" (first part, page 22), but never

[1] Bredichin found by his method for absolute personal equation from real stars the following values for his personal equation in the two positions: observer east, first series −0.004, second series −0.015; observer west, first series 0.195, second series 0.241. The observations were by eye and ear on equatorial stars.

Hilfiker found the difference between his personal equation by the two methods to be different in the two positions of the instrument: observer east 0.024 s, observer west 0.018 s. For experiments incidentally touching direction of motion see also Buccola: *La legge del tempo*, p. 162.

before worked up. To observers comparing for personal equation by means of this eye-piece, the stars would appear to move in the same direction, right and left, as with the ordinary eye-piece ; but to the one at the east branch, stars south of the pole would appear to move obliquely upward, to the observer at the west branch obliquely downward ;[1] for stars below the pole the directions would be reversed. From Littrow's tabulated results (observations by eye and ear only) I take a few for illustration :

D — E (ORDINARY PERSONAL EQUATION — 0.07).

	D east. s.	Stars.	D west. s.	Stars.
1853—Oct. 14	0.00	3	—0.21	3
20	—0.01	3	—0.17	3
Nov. 9	—0.24	3	—0.56	3

H — D (ORDINARY PERSONAL EQUATION + 0.13).

	H east. s.		H west. s.	
1852—Feb. 10	+0.27	3	+0.15	3
12	+0.28	2	—0.04	2
Mar. 12	+0.07	3	+0.11	3

R — D (ORDINARY PERSONAL EQUATION + 0.50).

	D east. s.		D west. s.	
1852—Jan. 23	—0.69	4	—0.52	4
April 2	—0.89	3	—0.47	3
26	—0.65	3	—0.21	3

In 1870 and 1871 Wagner incidentally made determinations with motions toward the right and left in studying his absolute personal equation with a Kaiser machine.[2] His experiments with the two directions were unfortunately not made at the same time and do not show constant differences. The figures, however,

[1] Zenith stars would of course seem to move straight up and down.
[2] *Observations de Poulkova*, Vol. XII.

are given below in the section on the effect of rate (page 288) and may there be consulted.

A few years later Campbell and Heaviside, in connection with longitude work in India, also found differences of personal equation depending on the direction of motion.[1] Their longitude observations were made by the chronographic method, and with an eye-piece that showed the stars in their actual motions —that is, stars north of the zenith with motion from right to left, and stars south with motion from left to right. The relative personal equation for the first was $C - H = 0.124\ s \pm 0.0087\ s$; for the second, $C - H = 0.040\ s \pm 0.0034\ s$, Heaviside observing a little earlier than Campbell. From twenty-one stars near the zenith observed by Campbell through half their transit as north stars and through the other half as south stars (the order being reversed, of course, to exclude error), the following equation results: $N - S = 0.077\ s \pm 0.0067\ s$, motion from left to right being observed a little earlier than that in the contrary direction.

The most recently published and most satisfactory investigation of this question, though confined to motions left and right in a horizontal direction, was also made at the Greenwich Observatory.[2] An artificial transit apparatus on the plan of C. Wolf's, but improved at many points, was set up in 1885, and in 1887 the regular transit observers made special determinations of absolute personal equation with it for the effect of rate and direction. From the report of these by H. Turner I quote the following table. All the observations were by the chronographic method:

[1] *Monthly Notices*, XXXVII (1877), 283.
[2] *Monthly Notices*, XLVIII, 4.

ABSOLUTE PERSONAL EQUATIONS WITH THE GREENWICH PERSONAL EQUATION MACHINE.

Observer.	Direction.	Rate one half as fast again as the equatorial.	No. of transits.	Equatorial rate.	No. of transits.	Rate at N. P. D. 43½°.	No. of transits.	Rate at N. P. D. 30°.	No. of transits.	Rate at N. P. D. 15°.	No. of transits.
		s. *s.*		*s.* *s.*		*s.* *s.*		*s.* *s.*		*s.* *s.*	
Turner.....	l.	−0.053 ± 0.013	6	−0.065 ± 0.015	6	−0.098 ± 0.020	5	−0.115 ± 0.028	4		
	r.	−0.063 ± 0.013	6	−0.043 ± 0.015	7	−0.048 ± 0.020	5	−0.045 ± 0.028	4		
Downing.....	l.	+0.094 ± 0.007	18	+0.064 ± 0.008	19	+0.045 ± 0.011	15	+0.036 ± 0.014	15	+0.052 ± 0.026	13
	r.	+0.072 ± 0.007	17	+0.075 ± 0.008	19	+0.065 ± 0.011	15	+0.076 ± 0.015	14	+0.137 ± 0.026	13
Thackeray....	l.	−0.042 ± 0.006	26	−0.077 ± 0.007	25	−0.108 ± 0.009	22	−0.158 ± 0.014	17	−0.109 ± 0.024	15
	r.	−0.044 ± 0.006	26	−0.036 ± 0.007	25	−0.062 ± 0.009	22	−0.069 ± 0.014	18	−0.019 ± 0.024	15
Lewis.....	l.	+0.020 ± 0.006	25	+0.003 ± 0.007	22	+0.003 ± 0.010	20	−0.008 ± 0.012	21	−0.077 ± 0.022	18
	r.	+0.013 ± 0.006	24	+0.021 ± 0.008	20	+0.014 ± 0.010	20	+0.036 ± 0.013	20	+0.093 ± 0.022	17
Hollis.....	l.	+0.221 ± 0.007	18	+0.256 ± 0.007	23	+0.260 ± 0.012	14	+0.316 ± 0.013	19	+0.457 ± 0.023	16
	r.	+0.234 ± 0.007	19	+0.267 ± 0.007	23	+0.274 ± 0.012	14	+0.349 ± 0.013	19	+0.528 ± 0.024	15

In this table *l* and *r* stand for left and right, the side toward which the motion took place. The plus sign indicates that the observation was too late.

It will be noticed that the first and third observers have personal equations with minus values, that is, they anticipate the true time of the transit by an amount greater than their simple reaction-times. Compared with that standard, the second and fourth also anticipate, but not enough to make the values of their equations minus. The fifth waits till he has seen the star actually bisected or a little past bisection. But all are alike in observing at the equatorial and slower rates a little later with motion in the less accustomed direction, namely, toward the right; the anticipators anticipate less, and the tardy one is a little more behind. The differences, however, are not very certain till the slowest rates are reached.

The same tendency is shown in observations on real stars. Since 1884 a portion of the clock stars have been observed at Greenwich with motion from left to right by means of a prism eye-piece. From these and altazimuth observations the following comparative tables are composed:

PERSONAL EQUATION WITH MOTION *r* MINUS PERSONAL EQUATION WITH MOTION *l*.

Stars at a little less than equatorial rate.[1]

Artificial stars.	Real stars.	No. of nights.
H T $+0.027 \pm 0.020$	-0.035 ± 0.011	12
A D $+0.014 \pm 0.012$	$+0.010 \pm 0.004$	70
T $+0.042 \pm 0.010$	$+0.063 \pm 0.012$	28
L $+0.017 \pm 0.011$	$+0.020 \pm 0.004$	72
H $+0.011 \pm 0.011$	$+0.010 \pm 0.004$	85

Stars at rate of N. P. D. 43½°.

Artificial stars.	Real stars (altazimuth).
H T $+0.050 \pm 0.022$	$+0.041 \pm 0.020$
A D $+0.020 \pm 0.011$	$+0.030 \pm 0.012$
T $+0.046 \pm 0.010$	$+0.039 \pm 0.017$
L $+0.011 \pm 0.010$	$+0.047 \pm 0.013$
H $+0.014 \pm 0.011$	-0.015 ± 0.009

[1] The column marked "No. of nights" gives the number of occasions on which stars were observed with motion from left to right; it is the custom to observe two stars every fine night in this way.

The experimental results and those with real stars show but little variation.

C. Wolf explains the difference of personal equation in eye and ear observations as a result of asymmetry in the eye, and substantiates it in his own case by experiment. Such a peculiarity in structure is possibly not uncommon. With the chronograph method there is little chance for the operation of such a cause. It might, however, come in, for those that anticipate, in making their estimation of the distance of the star from the wire, but no explanation is yet put forward.

Personal equation is influenced by the rate of the star.—I have already spoken of the experiments of Bessel and C. Wolf in this particular (first part, pages 13 and 37). Bessel's results were negative; Wolf's are summarized in the following table :[1]

Rate.	P. E.	Error of P. E.
s.	s.	s.
31.5	+0.141	0.014
40.3	+0.120	0.010
53.1	+0.108	0.012
86.9	+0.091	0.015

The personal equation decreases as the length of time required to traverse the reticle increases, that is, as the rate of the star decreases; but the uncertainty is least at a rate somewhat greater than the equatorial, and increases with change in either direction.

The study of Wagner mentioned in the preceding section was designed chiefly to bring out the effect of very slow rates and the difference, under those circum-

[1] The rate is given by the number of seconds required to cross the reticle; the equatorial rate corresponded to a time of sixty seconds. The four rates are averages of groups; not the identical rates at which observations were made. It should be remembered also that all of Wolf's experiments were made by eye and ear.

stances, of personal equation by the two methods of observation. The motion of the artificial star was from left to right in the first set of experiments, from right to left in the second. The following tables are abstracted from those that summarize his results;[1] the rates are indicated by the declinations to which they corresponded:

FIRST SERIES, 1870.

Declination.	Absolute P. E. by eye and ear.	No. of series.	Absolute P. E. by chronograph.	No. of series.	Eye and ear minus chronographic.
°	s.		s.		s.
17.0	+0.010	3	+0.090	3½	−0.080
40.1	−0.009	5½	+0.061	7½	−0.070
69.4	−0.025	8	+0.070	7½	−0.095
77.9	+0.086	6	+0.173	7	−0.087
85 52′	+0.03	3	+0.14	3	−0.11
86 27	+0.04	7	+0.31	7	−0.27
88 22	−0.20	7½	+0.32	7½	−0.52
89 1	−0.37	7	+0.17	7	−0.54

SECOND SERIES, 1871.

30.4	+0.057	7	+0.065	7½	−0.008
66.9	+0.029	8	+0.085	9	−0.056
77.6	+0.002	8	+0.111	8	−0.109
85 58′	−0.06	7½	+0.29	7½	−0.35
88 12	−0.13	8	+0.33	8	−0.46
89 1	−0.40	8	+0.11	8	−0.51

The personal equation with the chronographic method increases as the rate decreases, except for the fastest rate in the first set, where the trials were relatively few, and for the slowest rate of all. There is also an increasing tendency to anticipate in the eye and ear values of the second set, and the difference between the two methods is in both an increasing one. Results of the same general import as these by exper-

[1] In Wagner's tables the minus sign indicated an observation later than the phenomenon; the signs have here been changed throughout to facilitate comparison.

iment were obtained from observation of actual stars. It should not be forgotten, however, that most of these rates are extremely slow (the bisection here must have continued an appreciable time), and the results are therefore not strictly comparable with those for more rapid rates.

The latest experiments on this point, as on the last, are those at Greenwich, but they are not comparable with those of Wolf, because made by the chronographic method. The table on page 285 shows the effect of rate as well as of direction; there is little need of further comment upon them. For the first four observers the tendency to change is strongest when the motion is toward the left. The change is an increase in anticipation; the minus values become greater, the plus values less. With motion toward the right there is greater irregularity, but at the very slowest rate a tendency to delay. The last observer suffers an increase of personal equation with both directions of motion. Turner comments on the result of the study as follows: "There does not appear to be any general law representing the change of personal equation with rate. We might suppose the personal equation to be made up partly of error of bisection and partly of a constant error in time either deliberate or unconscious. We should expect the latter not to vary with the rate or direction of motion, and the former to vary inversely as the rate, and to change sign with the direction of motion. This is fulfilled approximately for all the observers except H, whose 'error of bisection,' amounting to nearly 1″.0, does not change sign with the direction of motion, so that in this case the observer apparently waits until the star has traveled about 1″ past the wire—whatever be the

direction of motion—and then 'hangs fire' for about 0.2 s before the record is complete. Such a well-marked exception rather precludes generalization."

Without attempting to give an explanation where Turner has declined, it may not be amiss to notice a single suggestive point. Gould[1] has remarked on the difficulty that those who anticipate must have in adjusting the amount of their anticipation to the changing rates of the stars. If they record for a slow moving star when it is as far from bisection as a rapid one would have to be, the amount of their anticipation expressed in time is increased. Something of the same sort, but of a contrary effect, would trouble those that allow the star to pass beyond the wire before recording. A slow-moving star would take longer in reaching the point at which they begin to record than a rapid one. Four of these observers anticipate, as we have said, and one delays beyond the bisection. Correspondingly, in the table we find, at least for the direction of most customary motion, an increase of personal equation with decreasing rates.

So the question stands. The fact that personal equation is affected by the rate is established, but not yet the law nor the manner of its influence.

The personal equation is affected by yet other circumstances of observation.—The preceding sections have been concerned with the influence of the character of the object; in this, I shall speak of several other factors—the position of the observer, the illumination of the reticle, the kind of key used, and the like.

In actual observation, the position of the observer changes with the declination, and so with the rate, of the star. It has therefore been felt by astronomers

[1] Cited by Gill from Gould's Transatlantic Longitudes.

that experiments on the effect of rate made, as in most cases, without change of position, did not fulfill the conditions. The point has, however, received some attention from the experimenters. C. Wolf[1] adapted his machine to observing when flat on the back, with the following result (series one and four were made in the normal position, for the sake of comparison; all were made by eye and ear): 0.113 s, 0.114 s, 0.125 s, 0.103 s. He thinks, however, that the difference does not prove a change depending on position, probably because his results were exact only to 0.01 s. Rogers[2] found that a very constrained position made his observations (chronographic) a little earlier (0.011 s in the mean) and, strange to say, somewhat more regular.

In 1879, Bakhuyzen suggested an apparatus for the observation of artificial transits in the positions of natural ones,[3] and in the past year Wislicenus, of the Observatory of Strasburg, has reported such observations made with an ingenious device of his own.[4] His absolute personal equation in the five positions is as follows:

Rate expressed as declination.	Zenith.	45°	Horizon.	−45°	Nadir.
	s.	s.	s.	s.	s.
11½°	−0.19	−0.18	−0.21	−0.13	+0.14
60	−0.01	−0.02	−0.05	+0.04	+0.50
80	+0.52	+0.48	+0.32	+0.41	+1.77

The amount, and especially the direction of the illumination of the field and the reticle have been found

[1] Loc. cit.

[2] Loc. cit.

[3] Vierteljahrssch. d. Astr. Gesell., XIV, 414.

[4] Noted in *The Observatory*, December, 1888, p. 443, where the following criticism is made: "It must be remembered, however, that in different positions of the telescope, the contacts of the recording apparatus [which moves with the telescope] might be sensibly different. Very little information is given as to the adjustment of this apparatus."

important for personal equation. C. Wolf thought the illumination of the field of little effect, but Newcomb enumerates the brighter field as one among other reasons that might co-operate to make day observations systematically earlier than those at night. Rogers found that he observed a few hundredths of a second later when the wires were faint than when they were bright.

R. Wolf, of Zürich, with his assistant, undertook a careful investigation of the effect of direction of illumination.[1] He was led to this by finding his relative personal equation with Hirsch to be of different sign and amount when determined at Zürich and at Neuchâtel. It was discovered that the direction was without influence so long as the position of the ocular exactly suited the eye of the observer. But if the ocular were drawn out or pushed in too far, the sign of the personal error changed with the side of the illumination, illumination of the field and of the reticle having contrary effects, and the amount (which could reach several tenths of a second) varied with the amount of displacement of the ocular. These differences were not found with double-sided nor with day illumination. In circumstances described in a later communication,[2] two images of the reticle could be seen, which changed their relative positions right and left as the direction of illumination changed and as the ocular was displaced inward or outward. These briefly mentioned papers, though having to do with instrumental conditions, are extremely important for all determinations of relative personal equation where the sources of error they point out have not been excluded. Per-

[1] *Astronomische Nachrichten*, LXXV, 71.
[2] *Astronomische Nachrichten*, LXXVI, 369.

haps in some such way as this the enormous difference between Bessel and Argelander, and between some other pairs of astronomers, and likewise the progressive change of relative personal equation, are to be in part accounted for.

Personal equation is somewhat affected by the kind of key used in recording. Schiff reacted to an electrical stimulus on the tip of the tongue by means of a telegraph key, an "astronomical key," and a "book key," with these results: telegraph key, $0.156\ s \pm 0.023$; "astronomical key," $0.164\ s \pm 0.016$; "book key," $0.130\ s \pm 0.010$. Tacchini found figures from observations on a Wolf artificial transit apparatus as follows: "astronomical key," $0.172\ s \pm 0.027$; "book key," $0.166\ s \pm 0.016$.[1] The key used in the Naval Observatory at Washington is something like a telegraph key, but of course held in the hand in observing. A small but genuine difference of personal equation is introduced if observers hold the finger at different heights above the knob when ready to make a record.

The study of Rogers included many circumstances besides those mentioned. Some of his results may be briefly mentioned here. A change of more than forty degrees in temperature, carrying the mercury below zero, produced no definite effect on his own observations, though a single set on each of two other observers showed a delay of 0.073 s and 0.107 s respectively. Fatigue had little effect, but records taken after sleep following fatigue were somewhat quicker. Sets taken at intervals during a fast of thirty hours were, on the

[1] Tacchini: Sulla equazione personale. *Rivista sicula di scienze, lettere ed arti.* Anno I, Vol. 2°, p. 382, cited by Buccola, La legge del tempo, p. 170. The "astronomical key" was a button held between the first two fingers and pressed with the thumb; the "box or book key" (*tasto a scatola od a libro*) of Secchi is pressed with all four fingers.

average, 0.032 s quicker than a normal set taken just before. The relative personal equation between Rogers and one of the other observers for a set of stars separated by from ten to fifteen seconds was 0.069 s greater than for a set separated by only two or three seconds. The relative personal equation for the same two observers was 0.023 s smaller for artificial than for natural stars.

The personal equation is influenced by circumstances more or less purely psychic.—The time of the perception of one sensation is affected by the regular recurrence of a second. And this is true not only in the eye and ear observations, where attention must be given to two, but also in the chronographic, where the second is merely accidental. H. Leitzmann discovered such an effect in chronographic records made within hearing of a loud ticking clock.[1] The seconds given by the clock were alternately too long and too short, the long being 1.05 s, the short 0.95 s. The tick that marked the beginning of each was also followed at about 0.35 s by another made with the forward motion of the second hand. Beginning with the first tick of the long second, the times were as follows: 0.00 s, 0.35 s, 1.05 s, 1.40 s.

Leitzmann was an anticipator, trying, as he says, "to make the feeling of movement that corresponds to the closing of the electric circuit in the key contemporaneous with the perception of the coincidence of the star and wire." In observing, he was accustomed, in addition to taking the transits of the vertical wires, to take also the star's declination by tapping his key when the star was midway between a pair of hori-

[1] Wundt's *Philos. Studien*, Bd. V, H. 1, S. 56.

zontal wires. In working up his results after the observations were complete, he found that those for declination grouped themselves on certain tenths of the second. In 129 observations the distribution was as follows :

Tenth of the second,	0	1	2	3	4	5	6	7	8	9
Distribution of obs.,	2	4	3	30	60	18	4	0	4	4

The last two-thirds of the time covered eighty-six observations ; seventy-six of them fell on 3 and 4, nine on 5, and one on 1. A connection with the clock beat was evident, and the observer went on to examine the more accurate transit observations for traces of a similar disturbance. Such he was able to find.

Assuming that the differences that remained after reducing the observations at the individual wires to the central one were due chiefly to the disturbance in question, he distributed the amounts so found in groups corresponding to each fiftieth of a second, averaged them, and plotted curves from the averages. In his curves, plus ordinates correspond to an increase of personal equation ; minus, to a decrease. The curves are somewhat irregular, but a typical one would show a short wave followed by a longer one, in each of which there is first a descending and then an ascending phase. The first wave would cover the first four tenths of the second, the other the remainder. The points of greatest personal equation fall near the beginning and end of the curve, those of least in the middle of the second portion. The variations that the curves represent amount at the greatest to about 0.054 s, and the mean probable error of each value to about 0.004 s. Leitzmann attributes the changes to the influence of the intruding stimulus on attention, which influence was undoubtedly strengthened in his own case by a

habit of semi-consciously counting the beats as in observing by the other method. Into the details of his exposition, some of which seem to me open to criticism, I shall not enter here.

In observations by eye and ear, many observers show a preference for some special tenth of the second, so that it recurs in their recorded observations far more frequently than it should by chance, and this "mental time scale" is often unchanged by time or circumstance.[1] Whether this is to be attributed to slip-shod habits of estimation or to some untraceable association like those that give numbers and letters peculiar characters in the minds of some people, or to some effect of the clock-beat like that just described, it is impossible to say without fuller information. Similar, but involving retinal asymmetry, is the personal difference that affects the reading of chronograph records from the record sheet, that is, the fixing the relative distance of the jag in the line that marks the transit from the other jags that mark the seconds.

Surprise is a psychological factor that is hardly to be expected in transit observations where the observer sees the star advancing in the field and knows precisely what he is to do when it reaches the reticle. It seems to be a fact, however, that records at the first wire are a little less certain than at the rest.[2] The transits of the wires after the first recur at intervals with which an observer in time becomes so familiar that he could even make some sort of a record at the remaining wires for a star that disappeared after

[1] B. Peirce: Proc. American Acad. of Arts and Sciences, IV (1860), 197.

[2] Argelander: *Vierteljahrss. d. Astron. Gesells.*, 1872, p. 16, referred to by Downing (*Monthly Notices*, XXXVIII), who adds figures for probable error at first and ninth wire in observations of the sun.

crossing the first. The knowledge of the exact time at which the transit is going to happen enables him to make full mental and physical preparation for its observation.

Knowledge of the amount and direction of the personal error may lead to change in it. C. Wolf was able by three months' practice to reduce his anticipation in eye and ear observations permanently from 0.30 s to 0.11 s. Rogers found the assumption that he observed too late sufficient to quicken his observations by more than 0.03 s. Wolf probably had, like Rogers, the expectation of realizing such a change under such circumstances, and that expectation may have co-operated in producing the result.

The foregoing exposition of circumstances under which the personal equation is variable probably does not exhaust the catalogue. Everything, physical or psychical, that influences the nervous mechanism influences in some degree the time of perceiving and recording the perception.[1] Psychological experimenters have found measurable differences in reaction times depending on attention, practice, fatigue, intensity of stimulus, effect of drugs, emotional states and disease, and other differences that some, at least, have thought due to grade of education, race, age, sex, quality of stimulus, and temperature.

I may sum up the general result of the preceding pages as follows: The personal equation by both methods varies from assignable and unassignable causes. Among the former are the nature of the object (disk or star), its magnitude, its direction and

[1] How responsive that mechanism is appears in such experiments as those of Lombard on the Knee-jerk, AMERICAN JOURNAL OF PSYCHOLOGY, Vol. I, p. 5.

rate of motion, and sundry instrumental and psychic conditions. The explanations of the changes, more or less conjecturally advanced, point to individual differences in the effect of irradiation, in the structure or action of the eye, in habits of anticipation or delay in observing, in the form of recording (Wundt), and in the direction of attention. It is impossible to establish by the observations and experiments described the certain causes of the differences found. The investigations of the astronomers have generally had the very practical aim of discovering the corrections to be applied to their observations. They have therefore tried to reproduce as nearly as possible the complicated conditions of their regular observations. It is for this reason that such a review as has been made is much more fruitful in problems for physiology and psychology than in generalizations. For the purposes of these two sciences the experimental conditions must be simplified and varied, not complicated and held to a typical form.

On the general question of the nature of personal equation itself apart from its variations, the experiments have been a little more explicit and the work of the astronomers and psychologists has come nearer together. It is hoped to treat of the nature and cause of personal equation in the concluding article of this series.

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

On the Relations between the Function, Structure, Origin, and Distribution of the Nerve Fibers which compose the Spinal and Cranial Nerves, being the Marshall Hall prize oration. WALTER HOLBROOK GASKELL. Read May 24, 1888. Vol. LXXI of the Medico-Chirurgical Transactions of the Roy. Med. and Chirurg. Soc. of London.

In this "Marshall Hall prize" oration, Gaskell has reviewed his work on the nervous system since 1881. Taking this account of the author as a guide, it will be worth while to run over his important contributions, despite the fact that some of them are already several years old.

In 1881 Gaskell was able to show, by the use of a new method, that the vagus nerve not only inhibited the rate and force of the heart's beat in the frog, but was also able to accelerate the rate and augment the strength of the contractions (Proc. Roy. Soc. XXXIII, 1881–2). A special feature of the method used was a clamp by which any desired pressure could be applied between two portions of the heart, and thus an artificial resistance to the passage of an impulse from one part to the other introduced. This led to the discovery that the vagus nerve in the frog was in reality a vago-sympathetic, the vagus fibers causing inhibition, and the sympathetic acceleration and augmentation; that the two sets of fibers were present both in the tortoise and the crocodile, and that in all cases the accelerator fibers reached the heart by the same course as in the mammal (Journ. Physiol. Vol. V, 1884–5.)

Further, Gaskell was led to the view that the nature of the action of these cardiac nerves was directly on the muscle, and therefore inhibition is something taking place in the heart muscle itself, and is not to be explained by the interference of nerve action outside the muscle (Cong. période. internat. d. sc. méd. Compt. rend. 1884, Copenh. 1886).

If the after-effects of stimulation are considered, it becomes plain that the result of vagus stimulation is to put the heart muscle in a state of repair in which it is capable of doing more work; while the accelerator, by exhausting it, brings about an opposite condition. Regarding, then, principally the constructive action of the vagus, and the destructive action of the accelerator, they are designated respectively anabolic and katabolic nerves (Journ. of Physiol. Vol. VII, No. 1). This is the most extensive and perhaps the most important of these papers.

The notion of a double nerve supply, similar to that in the heart, to the different tissues of the body, was supported by the study of the muscular tissue of the rest of the vascular system, the muscular tissue of the alimentary tract and of glands, and has led to the suggestion that probably all the tissues of the body are supplied with anabolic and katabolic nerves.

The question as to whether tissues are supplied in this way is but another form of the question whether trophic nerves exist, using the term in its most general sense; and it is well known how much attention has been given to that question of late. The fact, too, is not without significance that those who are working with Gaskell, or under his direction, are at present studying this very point of the double nerve supply of the various tissues.

While these physiological ideas were being worked out, Gaskell made an anatomical observation that has been of fundamental importance to his work. On examining the sympathetic cardiac fibers going to the heart of the tortoise, he was much struck by the difference between them and the vagus fibers; the sympathetic fibers being unmedullated between the heart and the ganglion from which they emerge, while the vagus fibers are medullated throughout their course. Here was a distinct morphological difference between the anabolic and katabolic nerves, and further investigation showed that it held good for all animals examined. In studying these two sets of nerves, and following them back to their origin in the central system, he developed the large number of new facts and new points of view which are contained in the paper last cited. It was known that the accelerator nerves for the heart passed out in the anterior spinal nerve roots. On examining the upper dorsal and lower cervical roots in the dog, they were found to be free from non-medullated fibers. At the same time it was noticed that the anterior roots of the dorsal region differed from those of the cervical, in containing a group of very fine medullated nerve fibers. These very fine fibers were found to form the bulk of the white Ramus communicans by which the sympathetic ganglion was in connection with the central system.

The course of the accelerator fibers was then clearly in the anterior roots, where they were represented by a group of excessively small medullated fibers, thence to the appropriate ganglion, where the medullary fibers ceased and were continued on as non-medullated fibers. Further study showed that in the ganglia it was the ganglion cells that gave rise to the non-medullated fibers, and that the small medullated ones reaching the ganglion from the cord were in connection with these cells, the exact mode of connection not being worked out. When it is said, then, that these fibers lose their medullary sheath on passing through the ganglion, it must be remembered that, strictly speaking, the medullated fiber probably terminates in the ganglion cells in one way, while its physiological equivalent arises from these cells in quite another way. The anatomical difference between anabolic and katabolic nerves, then, depended on the point where the latter lost their medullary sheaths. This connection between the white Ramus communicans and the structure of the anterior root led to an examination of all the spinal roots in the dog, with the result of showing that only where these bundles of small medullated fibers were present are there any white Rami communicantes or homologous structures—in all other cases the Rami communicantes are non-medullated. It would necessitate a lengthy digression to give the facts presented in connection with the visceral ganglia, and they are here neglected, as it is of more importance to get at Gaskell's idea of what constitutes a complete segmental spinal nerve, and see the relations of his view to the interpretation of cranial nerves with which he concludes. This

much may, however, be said regarding the small medullated fibers: 1. A cervico-cranial outflow of them occurs in connection with the vagus and upper roots of the spinal accessory nerves. 2. A thoracic outflow occurs between the second dorsal and second lumbar roots; and 3. A sacral outflow occurs in the roots of the second and third sacral nerves.

This marked inequality in the outflow leads Gaskell to a number of other considerations. In the first place, the gaps between these outflows are filled up by the plexuses for the anterior and posterior extremities—a fact which receives no special interpretation. The limits of the outflows correspond so closely to the limits of Clarke's column of cells that in Gaskell's opinion they force one to the conclusion that this group of cells is connected with the visceral nerves. The very roundabout course of the constrictor nerves of some of the blood-vessels and of the sympathetic glandular nerves has here an explanation, for they all come apparently from the thoracic outflow, and, losing their medullary sheaths in the ganglia of the main sympathetic chain, pass in various directions to their destinations. "Further," he adds, "I was able to point out how this appearance and distribution of small medullated fibers corresponded to known facts as to the distribution of the nerves governing the muscles of the alimentary tract; how the known cases of vaso-dilator nerves and the secretory nerves of the glands were all characterized by the fineness of the caliber of their fibers and the presence of ganglion cells on their course; that in fact a large class of nerves existed of various functions which might all be called visceral or splanchnic nerves, all characterized by the fineness of their fibers, and by the peculiarity that, although *efferent* in function, they were in connection with ganglia in some part of their course."

Looked at from this standpoint, the sympathetic (so called) and homologous ganglia appeared, not as the representatives of an independent nervous system, but as ganglia occurring in the course of the spinal nerves, and belonging to that system as much as the posterior root ganglia belong to it. The study of these ganglia on the efferent fibers shows them to differ from those on the afferent, in the fundamental fact that the fibers which emerge from them are non-medullated, while those which emerge from the latter are medullated. A means of distinguishing between the two sorts of ganglia is thus given, and the distinctions are utilized when Gaskell discusses the ganglia on the cranial nerves.

Formulating these facts, Gaskell describes a spinal nerve as consisting of an anterior and posterior root, both of which are ganglionated, with this difference, that the ganglion on the posterior root is comparatively stationary, occurring near its origin, while the ganglion on the anterior root is vagrant, occurring as a rule at some distance from the central system. Moreover, these motor vagrant ganglia are in connection with a special group of fine medullated fibers.

Further, there are in all probability sensory nerves which supply the region over which the ganglionated efferent nerves are distributed, and these might be considered as forming a special group in the posterior roots. Still another subdivision was introduced by the consideration of the relation which these fine ganglionated visceral fibers bore to the groups of large motor fibers controlling respiration, deglutition and expression, which are found specially in the spinal

accessory and the vagus group. These large fibers thus associated with the small ones corresponded closely with Bell's group of respiratory nerves. Gaskell gets his explanation for this relation of things from certain researches by van Wijhe on the mesoderm segments in the head of selachians.

In this case there is a double segmentation into a dorsal and ventral series of muscle plates, the dorsal series being supplied by the third, fourth, sixth, and twelfth nerves, and a ventral series by the fifth, seventh, ninth, and tenth nerves. The muscles developed from this latter series were those of mastication, respiration, and deglutition, thus belonging to the respiratory group. These coarse fibers that are found associated with the fine ganglionated efferent fibers seem, then, to form a special group. Designating fibers as somatic or splanchnic according as they supply the dorsal or the ventral mesoderm segment of van Wijhe, a spinal nerve may be considered as composed of: 1. A posterior root with a stationary ganglion into which both somatic and splanchnic afferent fibers pass; 2. An anterior root composed of (*a*) a large fibered somatic and (*b*) a large fibered splanchnic portion, both of them non-ganglionated, and of (*c*) a small fibered ganglionated splanchnic portion, the ganglion of which is vagrant.

There are therefore three groups of fibers recognized in the anterior root and two in the posterior, the idea being best supported by the arrangement of the fibers in the upper cervical and lower cranial region.

The groups of cells in the cord are harmonized with this division of the nerves in the following manner. Dividing the cells into somatic and splanchnic, it is found that the somatic efferent and the somatic afferent arise respectively from the ends of the anterior and posterior cornua. The splanchnic afferent are supposed to have their center in the solitary cells of Schwalbe in the neck of the posterior horn. The non-ganglionated splanchnic efferent fibers seem to be connected with the cells of the intermedio-lateral tract situated in the lateral horn, while the small ganglionated efferent fibers are made to arise from the cells of the column of Clarke.

As the discussion of these centers of origin is, however, not fundamental to what is to be said on the homologies of the cranial nerves, it may be left at this point. In the study of the cranial nerves there are a few general points that serve as guides. A complete segmental nerve should exhibit the five groups of fibers just enumerated. It must be determined whether the ganglia on them are stationary or vagrant. It must also be borne in mind that the ventral roots do not need to run free of the stationary ganglion, but may even run through it, as, for example, is the case with the first two cervical nerves of the dog.

Gaskell does not consider his work on the cranial nerves as yet complete, but is willing, nevertheless, to put forward some general conclusions. In the first place, the olfactory, optic, and auditory nerves are left entirely out of consideration. The remaining nine nerves seem to fall into two groups: 1. A foremost group, which in man is almost entirely efferent, namely, the oculomotor, trochlearis, motor part of the trigeminal, abducens and facial; 2. A hindermost group of nerves of mixed character: sensory part of the fifth, glosso-pharyngeal, vagus, spinal accessory, and hypoglossal.

The nerves of the first group resemble the anterior roots of spinal

nerves, in that they contain the three groups of fibers considered as typical. Gaskell is inclined to count them, however, only as four nerves, the abducens being regarded as the somatic portion of the motor part of the trigeminal. This foremost group of nerves also resembles a typical spinal nerve in its posterior root, in that they have a ganglion near the place of exit of the nerve from the central system. This ganglion, and the fibers connected with it, are in all cases phylogenetically degenerated, and a characteristic arrangement of connective tissue is all that is left to show where the nerve structures have formerly been. For the third, fourth, and seventh this condition is shown by figures of sections in an accompanying plate. This group resolves itself, therefore, into four segmental nerves, the afferent parts of which have undergone degeneration. In the second group above named there has been no loss of any part, but there is at the same time considerable dislocation of the various components, so that it requires some rearrangement to recognize the five segmental nerves which Gaskell considers to exist here. His statement of the case is this:

"1. The ascending root of the trigeminal, together with the auricular branch of the vagus, contains the somatic afferent nerves. The ganglion for these nerves is mainly the Gasserian ganglion.

2. The ascending root of the glosso-pharyngeal and vagus nerves contains the splanchnic afferent fibers, with the ganglion jugulare for its stationary root ganglion.

3. The hypoglossal, and probably part of the spinal accessory, form the somatic efferent portion.

4. The large motor fibers of the glosso-pharyngeal (?), vagus, and spinal accessory in part form the splanchnic non-ganglionated efferent fibers.

5. The small fibers of the glosso-pharyngeal, vagus, and spinal accessory, with the ganglion petrosum glosso-pharyngei and the ganglion trunci vagi for their motor ganglia, form the splanchnic ganglionated efferent fibers."

From these facts it is plain that the cranial nerves are built upon the plan of the spinal nerves, but are divisible into two groups, the meaning of which is yet to be made out.

Thus far what has been presented has been almost directly quoted from this last article of Gaskell. In calling attention, now, to those points in the argument for which the evidence appears insufficient, or is contradictory to apparently well established results, it is proper to speak only in the most general terms. There is certainly a gap that needs filling, between what has been determined for the anabolic and katabolic nerves of the heart and some other organs and the generalization that all the tissues of the body have a similar double supply. The voluntary muscles at once present themselves as a problem in this connection, and thus far two sets of nerves have not been shown for them. It is, perhaps, suggestive that all those cases in which the double supply is known to exist are in organs not under the control of the will. The association of the small white fibers in the anterior nerve roots with the cells of the column of Clarke, on the basis of the coincident extent of the two structures, is a view so very contrary to that now held concerning the connections of Clarke's column, that it must be supported by stronger evidence than that presented before it can hope for acceptance.

There is, further, no example of the typical spinal nerve as it has

been described. The typical nerve is a composite made up from the consideration of nerves at several levels, and finds its best exemplification in the cranial region which is apparently most modified. The central termination of the other two splanchnic groups has but very little positive evidence in favor of it thus far. In explaining the homologies of the cranial nerves, it is without doubt the nerves of special sense that are the most difficult to bring into line; but it certainly is an open question whether any arrangement of the nerves which leaves them out of account, as that of Gaskell does, is to be looked on as more than tentative. Supposing, however, that these criticisms should be supported by the results of future experiments, there would still remain a most important mass of information which has been the direct outcome of Gaskell's work. In the first place, the idea of anabolic and katabolic nerves, whatever the extent of its application, has a very considerable explaining power. To the neurologists, however, the fact that the so-called sympathetic nervous system can be shown to be an integral part of the spinal system, and not something independent, is of the utmost consequence; for not only is it a distinct anatomical advance, but, like all well grounded anatomical ideas, it disposes of much antiquated physiology of the region. The light which these ideas throw on the cranial nerves has certainly made it possible to attempt some order among this difficult group, and whether Gaskell is exactly on the right track or not, it seems clear that their illuminating power has not been exhausted.

Anleitung beim Studium des Baues der nervösen Centralorgane im gesunden und kranken Zustande. Dr. HEINRICH OBERSTEINER. Leipzig und Wien: Toeplitz & Deuticke, 1888.

In discussing a book that thus purports to be an introduction to the study of the central nervous system, the present state of the literature must be kept in mind. With the development of histological technique, the finer anatomy of the brain and cord began to be studied, and accordingly the last thirty years embrace most of the papers on minute structure. Of course this branch has been subject to the same laws that apply to all histological work, and the rapid increase of contributions in the later years of this period has been enough to appall any author contemplating a book that should represent the present state of knowledge on the subject. The disconnected character of the majority of the contributions which state the anatomical conclusions of the author and leave to some future compiler to knit the results together with those of others, is but the consequence of the comparative absence of general notions under which these special observations can be classified, and of course makes compilation most difficult. Various authors have attacked the problem in different ways. Schwalbe has produced something like an encyclopaedia of the subject. Henle, Meynert, and Wernicke have given rather individual accounts, less controlled by the views of others than is desirable in a manual. The excellent "Zehn Vorlesungen" of Edinger lacked sufficient detail for use in the laboratory, and, it should be added, was not intended for that purpose. There was room therefore, and need as well, for a book that should give a general view and yet contain sufficient detailed description to be used in the laboratory beside the microscope. No doubt Obersteiner is not the first man who has felt the desirability of such a

work, but, to most men, the combination of the continuous narrative style which was demanded in such a book, with a full treatment of the evidence, seemed quite incompatible. In meeting this difficulty, Obersteiner has followed the plan of taking what seemed to him the best view, and presenting it, as a rule, with little comment, only now and then stating the more important deviations from it. The plan is simple enough, but to carry it out to the satisfaction of the reader and the illumination of the subject implies a delicacy of anatomical instinct that is, to say the least, rare.

The book is divided into seven sections. The first discusses the methods of investigation under the five subheads of fibering, serial sections, pathological changes and arrested development, the method of comparative anatomy, and the experimental-physiological method. The value of each is briefly stated. When speaking of the method of serial sections he describes the histological technique so far as it applies to the nervous system, and gives a very compact statement of the methods of staining, etc. There is a blemish in the presentation of some formulae, however, that should not occur in a book of so excellent a character, that is, the "cook book" recipes for making various solutions. For example, one is instructed (p. 11) to take "as much haematoxylin as will go on the point of a knife." The thing is too trivial for further comment, but certainly important enough to deserve correction. In explanation of his division of methods, it may be said that by the experimental-physiological he means to describe such a method as was used by Ferrier and Yeo in studying the distribution of the spinal nerves in the monkey, and not the methods of degeneration which are sometimes understood by that term. Further, the methods of v. Gudden, Waller, and Flechsig are all summed under the general head of pathological changes and arrested development.

In the second section, after a couple of pages on the vesicles of the brain, is a description of the gross anatomy of the brain, most admirably illustrated, and followed by an account of the convolutions, in which Ecker is mainly followed. In reading this section the attention is at once fixed by the lucid style of the author, which gives to the descriptions a simplicity and clearness that is not always a distinguishing feature of books on this subject. The impression is unavoidable that the man is writing with the specimen before him. This charming clearness comes out even more strikingly later on, but it may be sufficient to mention it here, adding, though not without fear of a smile from the incredulous, that, by virtue of the form in which it is here presented, the anatomy of the nervous system becomes positively entertaining. It has previously been maintained in these columns that the proper understanding of the structure of the adult brain could not be gained without a clear grasp of its embryology. The absence of a sufficient discussion of the development of the brain seems to us the main and the sole important defect in the book. Perhaps the fact that one may read the book without feeling the lack of the information here demanded is in itself an answer to the above criticism, but we cannot suppress the feeling that a discussion of the embryology would have been of much value to those who will use the book. The descriptions of the plates are full and clear, but there is no order in the arrangement of the descriptive terms, and one is often quite discouraged in searching for a name. Wilder seems to have reached the best solution of

this problem, which is really a serious one for all parties concerned: he has arranged the terms alphabetically. It would certainly save time and vexation if some such plan could be adopted here.

In the third section the histological elements are taken up, the description of the nervous elements being followed by that of the non-nervous. Obersteiner here pursues the very instructive plan of describing at the end of each subdivision the pathological changes to which the element is liable. Nothing could be more to the advantage of the student, for not only is pathological material the most abundantly used in the laboratory, but it is as a rule the most instructive, and these few hints as to the significance of the changes are highly useful. The plan of giving the pathological variations is followed in the later sections, and is rather a distinctive feature of the book. In treating of the nervous elements he fails to use the idea that the nerve fiber is an outgrowth of the nerve cell, and that therefore fiber and cell form a unit, but takes rather the traditional course of considering them separately, without indicating such a connection. This is the more remarkable, since not only are the views expressed based, as a rule, on the very latest results, but all the way through there is an evident desire to give prominence to such ideas as have a generalizing value. It is in this section that the results obtained by Golgi come in for discussion. He follows that author on the relations of the protoplasmic processes of the cells and the lack of direct anastomosis between them, but seems almost ultra-conservative on the question of the axis cylinder as brought out by Golgi's method.

The finer structure of the spinal cord, which is the subject of the fourth section, contains the unorthodox argument for the origin of the fibers of the posterior spinal roots directly from cells and not indirectly from the fiber network, as is usually taught. It seems probable that if the facts are regarded in a certain way, Obersteiner's view should be added to the older one, but it can hardly be granted that the new view displaces the old one, as the paragraph indicates.

The next forty pages, forming the fifth section of the book, will be without doubt the most useful portion of the book. It contains the description of some twenty excellent woodcuts based on carmine preparations. The first cut represents a level of the cord just at the commencement of the crossing of the pyramids, and the last is through the middle of the optic thalami, the intermediate sections being taken at the most instructive levels. The location of each section is designated by lines transverse to the representation of the medulla and the basal ganglia. In the cuts themselves one half is made schematic and the other a copy of the actual preparation; a combination which has much in its favor. It is in this section especially that the freshness of the descriptions offers such a pleasant contrast to the portion of most treatises with which it can be compared.

As might be expected, the sixth section is the most extensive, since it comprises a discussion of the tracts in the cord, the cranial nerves, the forebrain, etc. It is in discussing the cranial nerves that there is the greatest opportunity for the display of an anatomical instinct which may enable one to select from the mass of contradictory results, all of which seem about equally well supported, those which are really valuable. The narrative here is crisp and clear, and the confusion brought about by a multitude of names for

the same thing is quite dissipated by the simple device of describing the thing itself and leaving the names quite at one side. There are probably several people in the world who will feel greatly indebted to Obersteiner for his description of the lemniscus and of the centers of the cranial nerves. This is, perhaps, the place where the kind of evidence which the author is willing to accept is most clearly recognizable. We do not know that Obersteiner was a pupil of Meynert, but certain it is that he has not that feeling of skepticism towards conclusions based on observation of the normal tissue which is felt by the followers of von Gudden, for example. He is conservative always, but at the same time is willing to interpret much that will have to be demonstrated at some later day. The book terminates with the seventh section, on the envelopes of the brain, followed by a good index.

By what has just been said concerning the kind of evidence that appeals to our author, we would not be understood to impute one-sidedness, save so far as every man has some bias in that he does not exactly agree with his neighbor. It is eminently a spirit of fairness that characterizes the book, and it is quite free from the narrowness of a special school. No one method or point of view can give a satisfactory survey of the entire field, and Obersteiner is far beyond the not uncommon attitude of mind of those who mingle patriotism with science, and look upon the method discovered in their town as the only correct one, or at least the most correct. The book, then, is a laboratory manual of unusual excellence, and, at the moment, is the only one of its kind.

Pathologie und Therapie der Nervenkrankheiten für Aerzte und Studirende. Dr. LUDWIG HIRT. Erste Hälfte, S. 256. Mit zahlreichen Holzschnitten. Wien und Leipzig: Urban und Schwarzenberg, 1888.

This first half of the book deals with the brain, and is to be followed by a second half, of the same size and character, on the spinal cord. Some description of the first half will show the character of the book, for the portion which is to follow is to have the same general construction. The author has made his compilation compact, and avoids the discussion of the more unsettled questions. The arrangement is highly systematic. There are three sections, dealing respectively with the diseases of the brain envelopes, the cranial nerves, and the brain substance; each cranial nerve, for example, is discussed in a separate chapter. The chapter is opened by a brief anatomical description, followed by the diseases, diagnosis and treatment, and terminated with a very fair collection of references to the literature. The book is well illustrated throughout, by cuts from standard works and a few that are original.

On Some Results obtained by the Atrophy Method. E. C. SPITZKA and R. MOLLENHAUER. Journ. of Nervous and Mental Disease, N. S. Vol. XIII, No. 6, June, 1888.

In a kitten two days old the left crus cerebri was severed by means of a cataract needle, which was inserted through the skull and pressed downwards and outwards at a point in front of the anterior pair of the corpora quadrigemina. The animal was killed just ninety days later. At the time of operation it was two days old and

the eyes were closed. It first showed strong circus movements to the right and had always to be fed. Its development throughout was retarded, and although at first it was playful, it later became rather sluggish. When the brain was examined it showed the left hemisphere, thalamus, oculomotor nidus, and associated parts completely atrophied. The other hemisphere was, as usual in such cases, more than the normal size and extended over the middle line. The optic tract, the oculomotor root and the pyramid on the left side had all atrophied, while remaining normal on the right side. The corresponding corpus quadrigeminum and the optic nerve had diminished in size in accordance with these atrophies. On sections through the region of the pons, the mesial and intermediate portions of the lemniscus, the posterior longitudinal fasciculus, and a crossed bundle of large fibers near the raphe have atrophied to various degrees. Passing caudad, the atrophies become less and less marked. When compared with certain results obtained by Forel and v. Monakow, the *plus* of atrophy may be attributed to the destruction of the thalamus, and thus the course of certain tegmental fiber systems is established.

Comparison of the Convolutions of the Seals and Walrus with those of the Carnivora, and of Apes and Man. Sir WILLIAM TURNER. Jour. of Anat. and Phys. Vol. XXII, 1888, pp. 554–581.

Sir William Turner, in his report on the seals collected during the voyage of the Challenger (*Zool. Chall. Exp.* Part LXVIII, 1888), describes the brain of the elephant seal (*Macrorhinus leoninus*) and of the walrus (*Trichechus rosmarus*). In connection with this description, he compares the cortical areas of the cerebrum in these animals with those found in the Carnivora proper, and in apes and man. After referring to the accounts given by Lauret, Broca, Owen, and Krueg, he describes the fissures and convolutions of *Phoca vitulina*. On the outer surface of the hemisphere in this mammal is a distinct fissure of Sylvius, with its Sylvian convolution, the anterior limb of which is narrower than the posterior, and at its commencement concealed within the fissure of Sylvius. In the walrus, and also in the eared seal, bear, otter, coati, badger, and ratel, this narrowing and depression of this limb of the Sylvian convolution exists. The convolutions and sulci of *Macrorhinus* correspond in essential points with those of *Phoca*. From an examination of the brains of *Trichechus*, *Phoca*, and *Macrorhinus*, Turner is inclined to consider these animals as approximating, in the arrangement of the convolutions of the outer face of the hemisphere, to those carnivora which possess four tiers of convolutions in relation to the fissure of Sylvius, this arrangement being present in the dog, jackal, fox, and wolf. He found that the area named by Mivart the ursine lozenge was rudimentary or not definitely defined in the seals and walrus. Turner next compared the convolutions on the mesial and tentorial surfaces of the hemisphere in the Pinnipedia, with the corresponding ones in the brains of several of the Canidae and Felidae. The crucial fissure, he found, varied materially in its position in the Carnivora and Pinnipedia. In the seals and walrus it was so far forward as not to be seen on the dorsum of the hemispheres, but only at the anterior end of the cerebrum. In the cat and tiger it was visible in about the anterior fourth of the dorsum of the hemispheres; in the

dog, weasel, ferret, and coati, at the juncture of the middle and anterior third; in the badger, polar bear, and ratel it was further back, just in front of a line dividing the dorsum of the hemispheres into an anterior and posterior half. In comparing the cerebrum of the Carnivora and Pinnipedia with that of man and apes, Turner finds a morphological correspondence between certain of the convolutions and fissures. In the walrus and seals the Island of Reil, he says, may find its representative in the anterior limb of the Sylvian convolution, which is more or less hidden within the fissure of Sylvius. If this indication be true, he believes that the Island of Reil, which in the brain of the ape, and more so in man, is entirely concealed within the Sylvian fissure, is either the homologue of the Sylvian convolution of the carnivorous brain, or that the Sylvian convolution in the Carnivora potentially represents both that convolution and a rudimentary insula. He thinks there can be no doubt that the anterior and upper part of the splenial fissure in the brain of the Carnivora and Pinnipedia corresponds with the fissure which is known as calloso-marginal in man and apes. In several of the Canidae the splenial fissure was continuous with the crucial fissure, but in the cat, tiger, coati, and polar bear they were not continuous; whilst in *Phoca vitulina* the two fissures were continuous in one hemisphere, but not in the other. From the anatomical data and experimental evidence Turner thinks it may be assumed that the fissure of Rolando is homologous with the coronal fissure in the carnivorous brain. He says in conclusion: "From the point of view of the hypothesis of evolution there would be no reason to think that the smooth-brained lower apes had originated out of the Carnivora, at least after the cortex of the cerebrum in this latter order had begun to assume a convoluted arrangement. If they had been derived from a carnivorous animal with a convoluted brain, then in all likelihood the convoluted character of the cerebrum would not have disappeared in the process of evolution. If the higher apes have been derived by descent from the lower apes, then the hemispheres in the former, with their complex arrangement of fissures and convolutions, have been evolved from a smooth-brained stock, and not from an animal with such an elaborate arrangement of convolutions as is possessed by either a dog or a seal. Hence, the acceptance of this hypothesis is not inconsistent with the fact that the convolutions of the brain in the apes assume from the first their own methods of arrangement, and not necessarily that of the orders of mammals with convoluted brains which are lower in the series. Beyond, therefore, a certain general correspondence in the arrangement of those fundamental parts of the cortex which serve a similar purpose in these various orders, one does not find it possible to determine the presence of convolutions arranged in a precisely corresponding manner in the brains of the Carnivora and Pinnipedia on the one hand, and of man and apes on the other." F. T.

The Morphology of the Vagus Nerve. Thomas W. Shore. Jour. of Anat. and Phys. Vol. XXII, 1888, pp. 372–390.

After giving the anatomy and development of the vagus nerve in Petromyzon, Elasmobranchs, the frog, Amniota, and the chick, and after some discussion of the morphological value of its various portions in these types, the author concludes that there is evidence to show, first, "that the vagus is a 'compound nerve,' but not in the

sense generally supposed; it is rather a compound of the visceral rami of the anterior spinal nerves, and of the remnants of the brain-ganglia and lateral cords of the nervous system of invertebrata, than of several metameric nerves." Secondly, "that the visceral part of the vagus of fishes includes the branchial nerves, and has arisen from a coalescence of the visceral rami of the anterior spinal nerve segments, the corresponding motor and sensory somatic branches of which have remained separate." Thirdly, "that the ganglia of the cranial nerves (5th, 7th, 9th and 10th) are the representatives of the brain lobes of Nemertea, and probably of the cerebral ganglia of Annelida and Arthropoda." Fourthly, "that the ramus lateralis is of extreme ancestral origin, and is equivalent to the lateral strands in the nerve plexus of Nemertea, to the main nervous system of Annelida and Arthropoda, and possibly also to the nerve ring of Coelenterata." Fifthly, "that a study of the vagus nerve throws light on the question of the chordate ancestor, and does not tend to support the views of Dohrn and his school." Sixthly, "that the value of the vagus in deciding the question of the segmentation of the vertebrate head has been much overrated."

F. T.

A Rudimentary Sense Organ.

(1). *Zur Deutung und Entwickelung des Gehirns der Knochenfische.* Rabl-Rückhard. Archiv. f. Anat. u. Physiol. (Anat. Abth.) 1882.

(2). *Untersuchung über das Gehirn der Petromyzonten.* F. Ahlborn. Zeit. f. wiss. Zool. Bd. XXXIX, 1883.

(3). *Ueber die Bedeutung der Zirbeldrüse.* F. Ahlborn. Ibid. Bd. XL, 1884.

(4). *Zur Anatomie u. Entwickelung der Epiphyses bei Amphibien und Reptilien.* H. W. De Graaf. Zool. Anz., March, 1886.

(5). *On the Presence and Structure of the Pineal Eye in Lacertilia.* W. B. Spencer. Quart. Jour. Micr. Sci., October, 1886.

(6). *The Parietal Eye of the Cyclostome Fishes.* J. Beard. Ibid. July, 1888.

(7). *The Pineal Eye in Extinct Vertebrates.* E. D. Cope. Am. Naturalist, October, 1888.

(8). *Recherches sur le développement de l'epiphyse.* P. Franchotte. Archiv de Biologie, Tome VIII, 1888.

Amongst human anatomists there has existed much doubt and consequent diversity of opinion concerning the function of the pineal gland or epiphysis of the brain.

Recently—within the last three years—we have advanced far towards a solution of the problem, by a comparative study of this so-called gland in the different classes of vertebrates.

In 1882 Rabl-Rückhard (1) was the first to throw some light upon the function of the gland. He suggested, from his study of its development in the trout, that the pineal body might represent an eye and was comparable to the paired lateral eyes, but failed to give properly the anatomical structure of this median eye.

Ahlborn (2 and 3) carefully described its structure in the fish Petromyzon in 1883; and in 1884, discussing the nature of the pineal gland, says he comes to the conclusion that it may be regarded as the rudiment of an unpaired eye, and to be compared perhaps to the median eye of the lancelet and of the tunicates.

In 1886 de Graaf (4) first conclusively showed that in one of the lizards (Anguis) the pineal gland is modified into a structure directly comparable to an invertebrate eye. He further showed that in amphibia and reptiles this body arose as a hollow outgrowth of the hind part of the first brain vesicle—thalamencephalon—that this outgrowth became constricted off distally into a hollow sphere, and the proximal portion formed a stalk connecting the sphere or bulb to the brain.

The general attention of naturalists was drawn to the subject by an admirable article of W. Baldwin Spencer in the Quarterly Journal for 1886 (5). He confined his research to an exhaustive study of the eye in the group Lacertilia, and within the same group found all stages of development; in some forms a simple outgrowth—evagination—from the upper wall of the brain, and in others developed into a well defined eye. As to the general form of the epiphysis he says, "The simplest form is seen in Platydactylus, where it has merely the structure of a hollow outgrowth running at right angles to the surface of the thalamencephalon until it reaches the dura mater lining the cranial cavity. In Hatteria, on the other hand, we have a form in which specialization is carried to its farthest extent, with the result that the epiphysis becomes modified into three parts, (1) a proximal part, still hollow, and connected with the brain roof, (2) a median solid pineal stalk serving to connect the former with (3), the distal portion differentiated into an optic cup. These forms may be taken as two extremes, the gap between which is filled up by various modifications."

In his examination of Hatteria he made out the following structures. An absence of pigment in the skin indicates the position of the median eye, and beneath it lies that organ in a foramen between the parietal bones. Beneath the unpigmented skin there is found a layer of connective tissue, which would prevent an image being formed in the eye, and indicates in this particular a considerable amount of degeneration, even in this highly organized structure. He compares the general shape of the eye to a cone with its base towards the surface and the pineal stalk or nerve connected with the apex. The walls of this vesicle are divided into (1) an anterior portion (base of the cone) which forms a lens, and (2) a posterior (sides of cone) which is the sensitive area or retina.

A study of this retina has yielded most interesting results and we cannot do better than quote the author's own words. He says: "The retinal elements are arranged in a manner typical of invertebrates, *i. e.* the rods lie on the inner side bounding the cavity of the optic vesicle, the nerve entering posteriorly and not spreading out in front of the rods. In the same vertebrate animal we thus find eyes developed on both vertebrate and invertebrate types, both being also formed from the modification of the walls of hollow outgrowths of the brain," and, it may be added, both arising from the walls of the embryonic fore-brain, approximating to the same place of origin in the early stages of development. The retina is found to consist of the following structures: "a rod-like layer of bodies enveloped in deep pigment," which rods show a remarkable specialization in the optic axis of the eye. Here they are elongated to twice their ordinary length, and are connected at their proximal ends (outer ends in relation to center of cup) with a group of nucleated cells, which cells are connected with the fibers of the optic or pineal stalk.

The retinal elements to the right and left of the axis are connected at their proximal ends with nucleated bodies, and these in turn send down processes into the pineal stalk. Other interesting structures are described, but for details his figures should be consulted. Longitudinal vertical sections through the head of the lizard show that the eye is connected with the epiphysis of the brain by a solid well-marked stalk—the pineal stalk. The elements of this pineal stalk or nerve have "much the appearance of those found at an early stage in the developing nerve of the paired eyes, that is, they resemble cells which are undergoing a process of elongation so as to form long fibers."

The pineal gland has been found in fishes, amphibians, reptiles, birds and mammals in varying stages of degeneration, reaching its greatest development in reptiles, and is least developed (or most degenerate) in mammals. In some forms, viz. the lizards especially, a foramen is left between the parietal bones. In others even this is obliterated, thus in the amphibian cutting off the bulb from the pineal stalk, and enclosing within the skull in mammals all that remains of the pineal eye.

There is an eye developed in the median line in the Tunicates and in Amphioxus, and since Kowalevsky showed in 1866—and his results have been fully verified—that these two groups of animals must be included in the same great group with the vertebrates, the temptation to homologize this eye with the pineal eye is very strong. And when we further see that in both these groups the median eyes develop from the upper surface of the nerve tube at its anterior end, the resemblance becomes very striking. On the other hand, when we see how easily any epidermal cell becomes specialized to receive light vibrations—so much so that this property is often ascribed as one of the general properties of protoplasm—we must be very cautious before we homologize these structures; especially so when they differ much in details. Mr. Spencer discusses this relation and concludes "that the eye of the larval Tunicates is probably homologous with the pineal eye, and that the eye of Amphioxus may be in no sense homologous."

In the *Quarterly Journal* for July, 1888, Dr. J. Beard (6) has given an account of the structure of the pineal eye as found in the Cyclostome fishes, including the forms Ammocoetes and its adult condition Petromyzon, and Myxine. This very ancient group of fishes was chosen "on account of their exceedingly primitive characters," hoping in this way to get some clue as to the phylogeny of the parietal eye.

He finds the structure strictly comparable to the eye of the higher forms, and in all these forms the pineal bulb is connected by means of a stalk to the brain. Distally the wall of the bulb forms an unpigmented lens, beneath which is a cavity filled with fluid, and the proximal wall forms a retina. The histology of the retina in Petromyzon is one of the most interesting results of his research. With a low power of the microscope it is seen to be made up of three layers: "(1) an inner layer of rods (inner in relation to center of eye), which also contains pigment; following this, (2) a layer of nuclei; and outside this, (3) a granular layer containing a few ganglion cells." The inner layer of rods (1) shows an exceedingly interesting structure, and, to quote his own conclusions, "the end elements are shown to be of two kinds, comparable, it seems to me, to those in

the retina of ordinary eyes. By far the most numerous are the long rods; but in addition, and between the latter, one finds a few cones." Both rods and cones contain nuclei, and are directly continuous with the nucleated cells of the nuclear layer (2). These cells are in turn continuous with the ganglion cells of the outermost layer (3), which send processes into the pineal stalk.

Beard's conclusions as to the method of evolution of the pineal eye are exceedingly interesting, but very hypothetical. He says, "I think the development does show that the parietal eye is a slightly later development than the paired eyes, and that the organ has developed in connection with the paired eyes." He starts with the views of Balfour and others, that the paired eyes once opened dorsally "on the surface of the unclosed neural plate," and as the nerve plate gradually sunk below the outer surface of the body, the "neural plate and eyes got shut in," and at the same time the eyes received light laterally, that is, from the sides of the body, or on the ends of their retinal cells which trail out into the nerves. Beard supposes that at the time of closure of the neural tube, a portion of the primary dorsal light-sensitive area became pinched off, as the median eye, which, as we have seen, would still receive its nerve supply from behind (invertebrate type); that by the involution of the optic cup of the lateral eyes they would receive their nerve supply on the same surface which received the light (vertebrate type).

Prof. Cope (7) has examined the skulls of some extinct vertebrates with reference to the pineal or parietal foramen. Referring to the skulls of two very old fishes, he says: "The structure of the primitive vertebrates strongly indicates the origin of lateral or paired eyes from a single median eye, such as is found in Tunicata." "Among North American extinct reptiles," he believes that Diadectes "relied exclusively on the pineal eye for the sense of sight," while in others in which the parietal foramen is closed, casts of the brain show an extemely large epiphysis, and at the same time in Belodon a communication with the orbit (of the lateral eyes) is established.

The work of P. Franchotte (8) on the epiphysis in Anguis covers about the same ground as that of de Graaf and Spencer on that animal.

Thus all the evidence which has come in points conclusively to the fact that the pineal body or gland developed primarily as a median pineal eye. A study of extinct forms shows a larger parietal foramen than found in existing forms, indicating in some, perhaps, a better developed pineal eye.

A study of living forms indicates an absence of function as an organ of sight, for in none are *nerve fibers* discovered connecting pineal eye with brain, though the nerve stalk is there. All the essentials necessary for sight are shown to have *at one time* existed, but the pineal body must now be relegated to the class of rudimentary organs. T. H. MORGAN.

The Pineal Body (Epiphysis Cerebri) in the Brains of the Walrus and Seals. Sir WILLIAM TURNER. Jour. of Anat. and Phys. Vol. XXII, 1888, pp. 300–303.

In this paper, which was read before the Royal Society of Edinburgh, in December 1887, Sir William Turner makes some interesting statements respecting the appearance, position, and relations of

the pineal body in the brains of the walrus and seal. In the brain of the former animal this organ is of unusual size, pyriform in shape, with the apex directed forwards to the optic thalami, to which it is attached. The base is free, and projects backwards so as to be visible, when the brain is examined from above, in the mesial longitudinal fissure between the two cerebral hemispheres. In *Phoca vitulina* the pineal body resembles that of the walrus, in possessing three surfaces and having its apex forwards. It projects behind the corpus callosum, and rests on the corpora quadrigemina and the anterior part of the middle lobe of the cerebellum, but does not appear between the two hemispheres of the cerebrum, when the brain is looked at from above, unless the hemispheres are drawn apart. The pineal body in *Macrorhinus leoninus* has the same shape as in *Phoca*, and possesses similar relations to the cerebrum and cerebellum. In the seals the epiphysis cerebri is larger than in mammals generally, and in the walrus it is about twice as large as in the seals. F. T.

II.—HYPNOTISM.

Eine experimentelle Studie auf dem Gebiete des Hypnotismus. Dr. R. v. Krafft-Ebing. Stuttgart, 1888, pp. 80.

Prof. Krafft-Ebing has studied in detail a single case presenting peculiar hypnotic manifestations. The subject is a young woman of neurotic ancestry, whose own career is typically hysterical. Fearing her father's opposition to a love-match with her cousin, she went to a convent, where she seems to have been hypnotized by some of the sisters and urged to steal money. Fearing detection, she escapes, earns a precarious living, assumes male attire and becomes a private tutor for several years. She is often severely ill with hysterical attacks. Symptoms of kleptomania, of sexual perversion, and of suicidal tendency are also evident. At the time of investigation she is hemianaesthesic, the right side being affected, including the sense organs; and is subject to hystero-epileptic attacks. She is easily hypnotized, and often falls into a somnambulic condition which Prof. Krafft-Ebing takes as autohypnotism. She rather objects to being hypnotized, and though her story is not perfectly truthful, simulation is regarded as out of the question. All the usual hypnotic phenomena can be well demonstrated in her case, but only a few of the most striking call for remark. The ordinarily involuntary functions seem especially controllable in her hypnosis. If the shape of a letter, a glass cylinder, or scissors be held against her skin, with the suggestion that the article is red-hot, a fully developed scar is formed in the shape of the object applied. The same can be taken away by a suggestion, and if transferred from one side to the other becomes reversed as in mirror-script. The magnet has a powerful influence over her, inducing contractures, but as this influence is shared by any object in contact with a magnet, and the magnet itself fails when not in the hand of Prof. Krafft-Ebing, suggestion (perhaps by temperature changes) is evidently the *modus operandi*. She easily takes attitudes appropriate to suggested emotions and *vice versa*. But the experiment with drugs at a distance à la Luys is entirely without success. She easily assumes foreign roles, making her actions and handwriting suitable

to the character suggested, and carries out fantastic negative hallucinations (*e. g.* that only the head and arms of a man are visible, the appearance causing great consternation), as well as post-hypnotic suggestions, with automatic accuracy. Her time estimates are also remarkably exact; she will sleep the number of hours suggested, and many of her crises have been averted by such suggestions. This study is a very valuable one, and leads the author to the suggestion-hypothesis as the clue to all the phenomena. The case is full of interesting details, and may be taken as the type of hystero-epileptic hypnotism.

Die Rolle der Suggestion bei gewissen Erscheinungen der Hysterie und des Hypnotismus. Kritisches und experimentelles. Dr. ARMAND HÜCKEL. Jena, 1888, pp. 72.

The points brought together in this brochure seem destined to be the ones about which the most interesting and important discussion in hypnotism will center; and to Dr. Hückel will belong the credit of first suggesting, in a convenient and systematic form, this important line of argument. The point at issue is the crucial distinction between the schools of Nancy and of Paris, between those who regard the psychic element of suggestion as the key to all the phenomena, and those who recognize physical influences as of particular and definite significance in the typical hypnotic manifestations. The chief points to be proved against the Charcot school are the production of all their characteristic phenomena by suggestion merely, without physical aid, and the explanation of how suggestion entered into the doings of the Charcot subjects. The former of these points has been frequently established. Any and all of those peculiar phenomena obtained by Charcot by a pressure here, a pass there, have unquestionably been produced by simple suggestion; and, moreover, the fact that the same manipulations have turned out differently everywhere else from what they do in Paris is of itself quite suspicious. Add to this that on new subjects, the same manipulations, if performed without the subject's understanding their object, will fail. The second point, however, is not so easily disposed of. How can we explain the "transfert," the action of the magnet, the effect of metals, the transition from one stage to another, hemi-hypnotism, and so on? All this, says Dr. Hückel, is either the natural guessing of the subjects or the unconscious suggestion of the operators. In several cases, gold alone of all metals brought about the desired "transfert"; the most precious of metals is, by an analogy not difficult to appreciate, regarded as most efficacious, and such a train of thought will probably be the same in nearly all minds; it is a predictable preference. That gold has no specific influence is proven by the fact that gold believed to be copper had no effect, and copper believed to be gold had. So when a magnet is brought out before a subject, the inference is not far off that something peculiar is to happen, and if the operators confine their attention to one arm, something begins to happen to that arm; then when attention is transferred to the other side, the inference is drawn that the result is to be transferred. When once this result is found to please the operators, and other subjects take the hint from this, a clique is unconsciously formed, and by mere contagion the phenomena take a definite and characteristic form. Thus the dozen patients who have demonstrated so much for Charcot have undoubtedly established a

certain *esprit de corps* that induces all to go through the same performances. The supreme importance of unconscious suggestion is not to be exaggerated; as soon as this influence is duly recognized, we may expect uniform results, and not before. The case is given of a patient, never before placed under the influence of the magnet, who was asked to come into the room when the physicians were busy, and where, finding but one chair vacant, she naturally seated herself upon it. It had been arranged that this chair was close against a closet in which was a powerful magnet, constantly in action during the *three-quarters of an hour* that she was kept waiting, but without any result. When, however, the magnet was placed at her elbow, even with the current off, a marked effect resulted; proving conclusively the subjective nature of this influence. To show the same with regard to a psychic "transfert," it was arranged that patient A came with patient B to the suite of rooms of operation, where, unknown to A, patient C was placed; A entered one door, was placed with her back to the door leading to the room in which she naturally supposed B had been led, but which in reality was occupied by C, B having been dismissed in the hallway. B suffered from choreic movements, and instructions were given to C (whom A believes to be B) to remain quiet and allow a magnet (not magnetized) to be placed at her elbow; the patient caught the idea, and contractions and movements soon occurred as though the ailment of the absent B had been transferred to A. Here is "transfert" obtained purely by spontaneous suggestion. The same explanation Dr. Hückel uses for all of the apparently physical phenomena of hypnotism, arguing everywhere that the results are expected, are according to an analogy which the subject appreciates, are quite *natural*, and that unconscious suggestion is the most fertile of all sources of error.

Der Hypnotismus und seine Strafrechtliche Bedeutung. Dr. August Forel. Zeitschrift für die gesamte Strafrechtswissenschaft, IX, 1, 2. Berlin and Leipzig, 1888, pp. 65.

Dr. Forel, the renowned alienist, gives a most admirable presentation of the main facts of hypnotism, for the benefit of the legal profession, who, he believes, will soon have to busy themselves with the possible criminal acts involving this condition. Dr. Forel is a warm adherent of the Nancy school, having derived his interest in the phenomena from Dr. Bernheim himself, and having succeeded in obtaining in Switzerland precisely the same results so brilliantly demonstrated at Nancy. While his exposition contains little that is new, it is extremely well arranged, abounds in accurate and helpful distinctions, and emphasizes strongly the affiliation of the hypnotic sleep with ordinary sleep. Suggestion is the secret of all the phenomena; when we go to sleep we do it by assuming an accustomed attitude, in an accustomed place, and so on. All this is an auto-suggestion. Education is largely a matter of suggestion skillfully applied; one teacher excels another in the art of suggestion. Some individuals readily act under the influence of another's advice or will; others are born to command, carrying with them a manner that enforces obedience; witness Napoleon. Even those who are hypnotizable do not altogether lose their individuality, and a criminal suggestion is more easily carried out by persons with a weak moral training. That, however, real dangers exist in this direction, Dr. Forel fully

believes, and suggests methods for the detection of such crime. But he admits the problem to be in an unsatisfactory condition, and that when once this method becomes general, the courts will have to act promptly. One of his remedies is to suggest to each patient that none but he (Dr. Forel) can hypnotize them. The main source of danger comes from the amnesia following a hypnotic suggestion; the patient believes that his motives are his own, and thus experimentally proves the dictum of Spinoza, that "the illusion of free will is nothing else than the ignorance of the motives of our acts."

Ueber Hypnotismus. Dr. HERING. Sammlung naturwissenschaftlicher Vorträge, Dr. Ernst Huth, II, 2. Berlin, 1888, pp. 16.

A somewhat rambling general address, describing in an unsystematic and uncritical tone the chief facts of hypnotism. The author represents no particular point of view, and has evidently not gone very deep into the topic. He was urged to give the address by the interest aroused in the phenomena by a traveling mesmerist.

Hypnotismus und Wunder. Ein Vortrag mit Weiterungen. MAX STEIGENBERGER, Domprediger. Augsburg, 1888, pp. 23.

Even the church has entered the arena of hypnotism. A passing analogy between the trance states to be found in hypnotics and the religious ecstasies of saints is sufficient to arouse in the author a fear lest the accredited church miracles will lose their hold upon the people. He therefore feels himself called upon to denounce hypnotism as partially the work of demonic agents, and to show the radical difference between miracles and hypnotic wonders. He argues that as long as hypnotism cannot explain *all* the wonders of church history, it is idle to consider it at all. One could hardly expect a sympathy of attitude or logic between the church and science on this point, but it is curious to see what shape this mutual misunderstanding takes.

L'Hypnotisme expliqué dans sa nature et dans ses actes. Mes entretiens avec S. M. L'Empereur Don Pedro sur le Darwinisme. Dr. CONSTANTIN JAMES. Paris, 1888, pp. 66 and 23.

It is with the first of these papers that we have to do. It contains a very curious collection of remarks upon hypnotism; a word or two on the nervous system; a clipping from the newspaper describing one of Dr. Luys's hypnotic seances, and so on. The author does not believe in hypnotism, and believes it creates diseases rather than cures them. It is dangerous to public health, it is immoral, and it is not a science at all. There is a good deal of deception about it, and the operators are the dupes. Hysteric subjects are not to be credited, and the whole movement is a "craze" that will have its day and be gone. Dr. James is a peculiar as well as a vague writer, and it is difficult to understand his point of view. Whatever is justifiable in his position seems to be grounded upon an opposition to the sensational and miraculous treatment of hypnotism now so prevalent in Paris.

Hypnotismus und Willensfreiheit. Vortrag gehalten in der Aula der Universität Basel von F. MIESCHER, Professor der Physiologie. 1888, pp. 36.

Like many such addresses, the object here is simply to acquaint the audience with the general features of hypnotism. This Dr. Miescher does by giving the history of the topic, accenting especially the work done by Dr. Braid and Dr. Liébeault. Then he describes the chief phenomena, anaesthesia, rigidity, supersensitiveness, negative hallucination, post-hypnotic suggestion, and so on, from the point of view of the Nancy school. Upon this follows a brief special consideration of the freedom of the will as illustrated by hypnosis. In this state we have the mind practically reduced to a state of automatism. Some amount of self-control, however, remains; and the suggestion of something entirely contrary to the subject's habits will be opposed. This automatism, too, is related to more normal instances of the control of one will over another. In the main, none the less the phenomena of hypnotism show how intimately the question of responsibility is connected with physical conditions, and how easily a condition of irresponsibility is induced.

Ueber hypnotische Suggestionen, deren Wesen, deren klinische und strafrechtliche Bedeutung. JOH. G. SALLIS. Berlin, 1888, pp. 53.

This very commendable pamphlet has for its object the instruction of the intelligent layman in the apparently mysterious phenomena now exhibited and commented upon all about him. The topics are treated in a plain manner, not exaggerating our present knowledge, and taking sound views on all doubtful points, such as the action of a magnet, of sealed drugs, and so on. The three stages of Charcot are not adhered to, and the position approaches that of the school of Nancy. The symptoms are treated under the heads of sensory, motor, and vegetative; the last including the influence upon involuntary organs. Many illustrative cases are cited, particularly in the therapeutic portions. On the whole the pamphlet is very similar to that of Obersteiner, though differing from it in many details.

Hypnotism or Mesmerism. CHARLES B. CORY. Boston, 1888, pp. 61.

America has as yet taken a comparatively small share in the discussion of hypnotism, now so all-absorbing in France and elsewhere. The present volume is a contribution from the chairman of the committee on Hypnotism, of the American Society for Psychical Research. It contains matter already published or read, and is intended to enlighten the American public on a topic in which much misconception exists. A general paper on hypnotism, partly historical and mainly expository, is followed by the most valuable of Mr. Cory's papers, in which he shows, by ingenious experiments, the part played by the voluntary yielding of the subject in the act of hypnotization. When, unknown to the subject of the experiment, he makes the most intense efforts to will her to sleep, no result is effected; while, if the patient is informed that Mr. Cory in another room is willing her to sleep, the desired effect takes place. Mr. Cory concludes: (1) that hypnotism is related to an abnormal constitution of the nervous system; (2) that only a small percentage of persons are hypnotizable; (3) that the condition is entirely due to

suggestion, no one being hypnotized without being informed or led to suspect that he is to be an object of experiment; (4) that the condition may be self-induced; (5) that in certain cases the hypnotic is insensitive. Quite as interesting are Mr. Cory's experiments on negative hallucinations, proving most conclusively that the subject takes some accidental peculiarity as the clue to not seeing a certain object, and that if that clue be removed the suggestion fails. The final paper illustrates the therapeutic value of hypnotism.

Ueber Beziehungen zwischen Hypnotismus und cerebraler Blutfüllung. HANS KAAN. Wiesbaden, 1885, pp. 35.

To the support of the vaso-motor explanation of hypnotism, the author brings two experiments, frequently repeated, and corresponding plethysmographic tracings. The only useful subject was a neuropathic woman of twenty-one years. If, when she was in the lethargic stage, a hot compress was applied to her head, she at once awoke. A cold compress had rather the opposite effect. But when she was cataleptic, the hot one was without effect, and the cold one caused a return to the lethargic state. The tracings showed a somewhat increased volume of blood in the arm in the lethargic stage and decreased in the cataleptic. These hot and cold applications, he believes, must have worked reflexly on the vaso-motor system, producing anaemias and hyperaemias in the cortex and lower centers. The author is not inclined to generalize from his single case, and with present information as to the subtlety of suggestion and the perceptive powers of subjects, would realize an alternative to the vaso-motor hypothesis.

Ueber hypnotische Erscheinungen. Vortrag von TH. MEYNERT. Wien. klin. Wochenschrift, Vol. I, Nos. 22, 23, 24 (1888).

Clinical observation and not experiment is, in Prof. Meynert's opinion, the true path to right understanding of hypnotism, and, though with much hesitation, in handling such a subject, he presents two cases from his own observation. In one, the patient had natural seizures, somewhat resembling states of hypnotism; in the other, the subject had been worked up by previous manipulators to a state of ultra-susceptibility. In the states of these two he finds a partial correspondence to the stages of Charcot, and explains what he finds, together with some of the more common hypnotic phenomena, on the basis of circulatory changes in the encephalon, local anaemias and hyperaemias, thus joining himself to an early view of Heidenhain, and one more recently supported by Kaan and others.

Magnetismus, Hypnotismus, Spiritualismus. Dr. GEORG V. LANGSDORFF. Berlin, 1889.

The trend of this pamphlet is spiritualistic and unscientific.

Ein Beitrag zur therapeutischen Verwerthung des Hypnotismus. ALBERT, FREIHERRN V. SCHRENCK-NOTZING. Leipzig, 1888, pp. 94.

From this pamphlet one gains an admirable idea of the extensive activity now absorbed by studies in hypnotism. The main portion of this thesis is devoted to a résumé of recent contributions to the therapeutic aspects of hypnotism in various countries. The two

longest sections are naturally devoted to France and Germany, but Belgium, Holland, Italy, Spain, England and America, Greece, Poland, Russia, Norway, Sweden, Denmark, Switzerland, Hungary, Austria, are all represented. The point of issue between Paris and Nancy forms a chief point of discussion, the tenets of each school being very clearly and fairly defined; the author deciding against the Paris views. But he is inclined to sympathize most strongly with the views of Fontan and Ségard, who, while siding in the main with the "suggestionists," leave some room for purely physical effects in the deeper stages of hypnosis. To the French belongs the credit of introducing new views into scientific circles, and studying strange phenomena with industry and enthusiasm; to the Germans the credit of introducing rigid criticism, severing the essential from the accidental, and bringing order into this young science.

Much space is devoted to statistics of cures by hypnotism. The large percentage of successful treatments guaranteed by so many who have used hypnotism leaves little doubt of its therapeutic importance. Its influence is most marked upon nervous diseases, though by no means confined to them; but Dr. Schrenck-Notzing is careful to add that it should not be used until other methods fail, and that it should be exclusively in the hands of experts. The motto prefaced by the author to his work might be taken as the watchword of the Nancy school; it reads "*Possunt quia posse videntur.*"

The author contributes reviews of hypnotic literature to the October, November, December, and January numbers of *Sphinx*.

Neue Heilmittel für Nerven. I. N. von Nussbaum. Breslau, 1888, pp. 16.

An address of rather miscellaneous content, describing various innovations in the hygiene of nervous ailments, massage, nerve-stretching, nerve-vibration, Swedish movements, and so on. Much of it deals with hypnotism as a therapeutic agent; the author rather indiscriminately choosing his facts, and incorporating much that is not at all certain along with the well ascertained. It is merely a popular presentation, lacking all originality.

Der Hypnotismus und seine Verwerthung als Heilmittel. Dr. E. Mendel. Die Nation, Jan. 14, 1888.

Dr. Mendel very briefly records his adherence to the Nancy school, cautions against a too free use of hypnotism as a curative agent, and points out the close analogy of hypnotic suggestion with more normal phenomena. Its curative effects stand on the same plane as all other psychic cures, should be as sparingly used, and retained in professional hands.

Origine des effets curatifs instantanés de l'hypnotisme sur les maladies chroniques. J. Delbœuf. Revue de l'Hypnotisme, Sept. 1888.

The point of the explanation put forward by Prof. Delbœuf is this: the first success in preventing a crisis by hypnotic suggestion acts as itself a suggestion against the next crisis, and that against the next, and so on, the force of the suggestion growing, as is common in hypnotic experimentation, with each success, till at last it is sufficient to overcome a strongly intrenched disease.

De l'auto-suggestion en médecine légale. BUROT. Revue Philosophique, Jan. 1889.

To the question of how to get at the suggester of crime when he has covered his tracks by suggesting amnesia as well, Dr. Burot makes the following contribution. He has found in a number of cases that if a subject, that has thus been made to commit a crime, is taught to hypnotize himself (auto-suggestion), with a view to recalling the forbidden circumstances, he finds himself free of the hindrance and able to do so, can relate the circumstances and identify his principal.

Casuistische Mittheilung auf den Gebiete der Suggestions-Therapie. Dr. E. BAIERLACHER. Münchener med. Wochensch. XXXV, No. 39, Sept. 25, 1888.

The author has tried suggestion upon 58 of his patients, 24 men and 34 women, between the ages of 16 and 71. 7 of the men and 8 of the women proved unhypnotizable, a considerably larger percentage than Bernheim's, but due partly to unfavorable circumstances. He records therapeutic failures in traumatic neuralgia, traumatic neurosis, neuralgia of both legs, apoplectic hemiplegia, persistent insomnia in emphysema of the lungs, and in extreme dizziness; successes in colicky pains following abortion, catarrh of the stomach, occipital neuralgia and neuralgia of the second branch of the trigeminus, irregular and painful menses (a case of each), and muscular rheumatism, besides minor ailments. In two cases he brought about a change of the pulse rate during suggestion, from 92 and 86 to 76; with a consumptive having a pulse of 120 he was unsuccessful.

Der Hypnotismus in der Geburtshilfe. JOH. G. SALLIS. Separat-Abdruck aus "Der Frauenarzt," 1888.

The author relates three cases, previously reported by others, in which hypnotism has been applied in first accouchements. All the patients had been hypnotized many times before the occasions in question, the first two with a view to trying hypnotism as an anaesthetic in labor. From the three it does not appear that hypnosis materially affects the regular course of parturition. In the first and third cases there was external evidence that the pains in the severest phase were actually felt, and in the second the patient was repeatedly awakened by them. The recollections of the pain were destroyed for the normal state. The author adds a brief bibliography of German works on hypnotism.

Dr. Van Renterghem, who, with Dr. Van Eeden, has opened an institute for the cure of nervous diseases by hypnotism, at Amsterdam, has gathered some statistics relative to the curative effect of the treatment. From May 5 to August 9, 1887, 178 patients were hypnotized; only 7 proved complete failures, and 20 became somnambulic at once. 162 were treated, of whom 91 were cured, 46 improved, and 25 unimproved. 37 different diseases were represented, of which the following is a selection:

	No. treated.	Improved.	Cured.	Not improved.
Rheumatic pains, ...	16	2	13	1
Various hysterical attacks, .	24	7	14	3
" neuralgias, ...	9	2	6	1
Epilepsy, ...	3	3	..	..
Indigestion, etc., ...	12	2	10	..
Deafness, ...	11	7	1	3

A review by Dr. Van Eeden, of the Swedish work of Dr. Otto G. Wetterstrand, *Om hypnotismens anvandande i den praktiska Medicinen*, in the *Revue de l'Hypnotisme*, November, 1888, gives some further statistics of the application of hypnotism in medicine. The results accord with those of the Nancy school, to which Dr. Wetterstrand belongs. From January, 1887, to the date of writing, 718 persons had been tried, of whom only 19 were found completely unhypnotizable. Special nervous condition and sex were not found to determine susceptibility; but character and especially age were important. Children are most easily influenced, and after thirty, susceptibility declines. This agrees with Dr. Van Eeden's experience, though he still believes that in very advanced age the sleep can easily be produced. No serious troubles were found to follow hypnosis, and unpleasant sensations that sometimes did follow could be put out of the way by suggestion. The long list of diseases treated contains many of an organic nature. A beneficial effect is alleged in the hemorrhages and diarrhœas of consumption, in heart disease (the improvement of the heart's action appearing in the tracings), in anæmia, and in Bright's disease. Though the last is also not without parallel in the experience of Dr. Van Renterghem and Van Eeden, the latter is still of the opinion that the treatment is most applicable in functional neuroses. Out of 74 cephalalgics, 65 were cured; of 26 stammerers 10. The least benefit followed in epilepsy, tabes dorsalis, grave psychoses and neurasthenia (with the last the Dutch doctors have been more successful); some benefit followed in *petit mal* and slight alienation, and much in alcoholism; in *enuresis nocturna* the success was constant.

Der Hypnotismus in der Pädagogik, von einem Schulmanne. Mit einem Vorwort von Joh. G. Sallis. Berlin, 1888, pp. 17.

This pamphlet, written by a well-known philologian, whose name is withheld, is prefaced by a few words from Dr. Sallis, supporting the conclusions therein maintained, and arguing great caution in introducing hypnotism as a reformatory measure into the schoolroom. He emphasizes the dangers to health and morality likely to accrue from an indiscriminate hypnotization; urges that in as far as it is an advisable process, pedagogy has other and better substitutes for it, and its only legitimate field is in the case of abnormal children, who at best need a special kind of training. In the preface Dr. Sallis hints at cases in which children have learned to hypnotize one another and use such power in the furtherance of vicious habits. Apart from a somewhat characteristically German opposition to a French proposition, this attitude towards educational hypnotism seems prompted by a wise conservatism and proper caution in dealing with new tools.

Emploi de la suggestion hypnotique pour l'éducation des enfants et des adolescents. LIÉBEAULT. Revue de l'Hypnotisme, Jan. 1889.

The answer to such questions as that of the applicability of hypnotism in pedagogy and moral reform lies in the pooling of observations. To that end Dr. Liébeault contributes a brief statement of 22 cases from his own experience. The subjects ranged in age from 14 months to 19 years. All except one were inferior in intellect, morality or emotional control; 13 were in good physical health, and 8 not so. Of the 13, three were set right, 7 were helped and 3 not helped (7 of these have not yet been long under treatment); of the 8, seven were set right and one not helped. A single case in normal physical and mental condition, a lycée student of sixteen, was able to work more and better while the treatment continued. In all, there were 10 cures, 8 betterments and 4 failures. Hypnotic suggestion proved useful in removing excessive fearfulness in 3, passion in 2, depraved habits in 4, lying in 1; two unruly children were somewhat though not completely restrained. An effort was made some time ago by M. Félix Héments to obtain official permission to put the thing to test on a grand scale among the inmates of the houses of correction. The results of such an experiment would be awaited with the greatest possible interest.

In an earlier number of the same journal (November, 1888), Dr. Aug. Voisin contributes the case of a youth of sixteen who from his boyhood had been uncontrolled, a liar and a thief, and had gone from bad to worse as he grew up. After exhausting his family and several reformatories, he was finally sent to Dr. Voisin. His bad habits gradually yielded to suggestion in successive hypnotizings, and at the end of a month he was apparently reformed. At the end of three months, six weeks after the last hypnotizing, he had had no relapse.

Des hallucinations négatives suggerées. Dr. BERNHEIM. Revue de l'Hypnotisme, Dec. 1888.

That a negative hallucination was a purely psychical act, a refusal to give audience to a sensation received and registered by lower centers, has long been an accredited doctrine of the Nancy school. Perhaps no more striking evidence in its favor has been furnished than that supplied in this article. The subject was a young lady of eighteen years, affected with sciatica, but otherwise without neurotic taint, easily hypnotizable and showing all the characteristic phenomena. Dr. Bernheim gives her the post-hypnotic suggestion that upon awakening she will not see him; he will be gone. Upon awakening he calls her, stands before her, sticks a pin into her skin, but she refuses to recognize any sensation emanating from him. It may be noted that this does not take place with every subject; some will simply not see the doctor, but will hear him and feel his touches; if such are told that they will neither see, hear nor feel Dr. Bernheim, the result is as that described. Desiring to see how far this condition admitted of abuse from a medico-legal point of view, Dr. Bernheim addressed insulting words to her and threatened violence. Ordinarily very sensitive and reserved, she gave no sign of feeling. She was then hypnotized and told that upon reawakening Dr. Bernheim would be there. This works well and Dr. Bernheim begins to question her. She

stoutly denies his having been present before, and seems to remember nothing of it. By dint of repeated and insistent declaration that he was present she at last remembers; then remembers what he did; and with great hesitation and blushing gives an account of what happened. This experiment shows that this negative hallucination is purely psychic, the impression being received and registered. It shows, too, that this apparent amnesia can in some cases at least be overcome and the latent, ignored impression forced into consciousness. Furthermore, it brings to light a condition of medico-legal interest: for here, in an apparently normal waking condition, a patient may be insensible to a suggested maltreatment.

Sur l'explication fournie par M. le Dr. Bernheim des hallucinations négatives suggerées. J. DELBŒUF. Revue de l'Hypnotisme, Jan. 1889.

The explanation of Prof. Delbœuf differs from that of Bernheim in the greater prominence that it gives to the co-operation of the subject. He holds, and fortifies his position by citing experiments that the subject behaves exactly as a waking person might that had determined to play the part to the letter. The case given by Bernheim shows nothing that might not have been done by such a person. "The subject lends himself to what is required of him with passivity, but with intelligence."

Recherches sur l'anesthésie hystérique. A. BINET. Comptes Rendus, CVII, p. 1008, Dec. 17, 1888, and Revue de l'Hypnotisme, Jan. 1889.

While skepticism is natural as to the total exclusion of suggestion which Dr. Binet alleges, his experiments are interesting from the likeness of his results to those of experiments on negative hallucination. The negatively hallucinated have been shown to see, hear, smell, etc.; it appears that these hemi-anesthesics really feel. The subjects were twelve hysterics in different Paris hospitals. The following are among the findings reported. Stimulation of an anesthesic area, which the subject was not allowed to see, produced no tactile or muscular sensation, but, instead, a visual image of the area. This could be projected on a screen, and lasted while the stimulation lasted. A prick was seen as a dot; figures drawn with the compass-point appeared in color; gentle constrictions of the wrist or finger called up the image of the part; passive movements were perceived as movements of the image, and could be counted. When two compass-points were applied, one or two dots were seen according to the separation of the points. Measured thus, the discriminative sensibility was found to be about normal. The separation of the points was correctly estimated by some patients if it did not exceed 2–3 cm., but larger distances were underestimated. The shade, light or dark, of the images varied with the subject; the color also changed as sensitive areas were approached. The images behaved in several particulars like after-images. Their details were clearly seen, but sometimes the image did not represent the part; for example, a passively moved finger might be seen as a baton or column. Familiar objects placed in the hand could be recognized as images on the screen. The patients are said not to have suspected the origin of the images, nor to have lost faith in their own anesthesia.

Auto-suggestione in sogno. LOMBROSO. Archivo di Psichiatria, Vol. IX, fasc. 4, p. 418.

In this brief note Lombroso gives the following case in confirmation of the likeness of normal sleep and hypnosis. He had frequently treated a certain neuropathic subject for severe neuralgia; the relief, however, not outlasting forty-eight hours. On one occasion, after suggestion against a painful toothache, the relapse fell in the night. The patient had, however, no sooner fallen asleep than he dreamed of seeing Lombroso, and of having suggestion made to him as usual. The pain ceased, and the relief persisted, like that secured in hypnosis, into the waking state.

Reaction-time in the Hypnotic Trance. WILLIAM JAMES. Proc. Amer. Soc. for Psychical Research, Vol. I, No. 3, Dec. 1887.

These experiments serve to show rather the variety of conditions that may exist in the hypnotic trance than to establish any law of the effect of hypnotism upon reaction-times. Two of the three subjects reacted more slowly in the trance than before it; one reacted in two series a little quicker, and in one a good deal slower. In three series, one on each subject, taken five or six minutes after waking, two showed a quickening even over the reactions taken before the trance; the other showed the same, but, unfortunately for generalization, relapsed into trance again before the set was complete. The average error appeared larger in the trance than in the waking state.

Bibliographie des Modernen Hypnotismus. MAX DESSOIR. Berlin: Carl Duncker's Verlag, 1888. 94 pp. 8vo.

This is a valuable and satisfactory bibliography of the whole subject of hypnotism. The books are entered in the bibliography with their full titles, and the articles have volumes and pages indicated. The division of the materials classified is as follows: I. General, 191 titles; II. Medical, 199; III. Magnetism and Hypnotism, 36; IV. Physiological, 62; V. Psychological and Pedagogical, 85; VI. Jurisprudence, 43; VII. Action at a Distance, 81; VIII. Modern Mesmerism, 58; IX. Various Topics, 46. The last including Preconditions of Hypnotism, Single Cases, Ecstasy, Hypnotization of Animals, Historical works, Theory of Hypnotism, and Hypnotism and Religion.

The statistical tables at the end show: 801 writings, 481 authors, 207 periodicals containing articles.

Of the writings (excluding translations) there are, in French, 473; in English, 102; in Italian, 88; in German, 69; in Danish, 22; in Spanish, 16; in Russian, 12, and from one to six in various other European languages. The growth of interest in the subject is exhibited in the following data: In 1880, 14 writings appeared; in 1881, 39; in 1882, 39; in 1883, 40; in 1884, 78; in 1885, 71; in 1886, 131; in 1887, 205; in 1888 (from January to April), 71. The work is concluded with a full index of authors. In co-operation with Dr. Bérillon, the bibliographical work is to be continued in the *Revue de l'Hypnotisme*. Portions have already appeared in the October, November, and January numbers. D. J. HILL.

III.—EXPERIMENTAL.

On the Seat of Optical After-Images.[1]

M. Binet, in his "Psychologie du Raisonnement" (p. 43 ff.), quotes from M. Parinaud an experiment which, he thinks, proves the cerebral, instead of the retinal, seat of optical after-images. The experiment is as follows:

"Close and cover with the hand the left eye, and with the right eye fixate carefully for some moments a small square of red or black paper on a white background. If now the square of paper be removed, as by blowing it aside, a negative after-image of it will be seen by the right eye on the white ground. Now at the moment of blowing aside the square, close and cover the *right* eye and open the *left* one. After a moment the white field will darken, and on it will be seen the negative after-image."

This formation of an after-image in one eye and its subsequent appearance on the field seen by the other is taken by M. Binet as complete proof that the after-image lies not on the retina, but in the common cerebral center of vision for both eyes. Thence it is projected into the field seen by whichever eye is open. But he quite overlooks the possibility that the image lies really on the right retina and mingles itself with the field of the left eye. He takes it for granted that the phenomenon is seen by the eye which is open, and not by the closed one. That he is wrong in his deductions, and that in failing to consider the possibility above mentioned he has missed the true explanation of the phenomenon, it is the purpose of this note to show.

A serious difficulty in settling the question lies in the well-known impossibility of separating the visual fields of the two eyes. Whether one eye or both are open, whether they are focused on the same point or are held parallel, or squinted, or even jammed into all sorts of relative positions by fingers inserted into their sockets, the field of each will appear to coincide with the field of the corresponding portion of the retina of the other. If an after-image be formed on both together, one image only will be seen, whatever their relative positions; and if the image be formed on one alone, it will yet be seen in the corresponding portion of the field of the other, provided the brilliancy of the second field be not so great as to obscure the much weaker sensation of the image. In reality, in this experiment the after-image never does appear on the left field until the latter has so greatly darkened as to allow it to be seen; and in the periodical increases in brilliancy of the left field the image disappears.

It will thus be seen that a retinal seat of the after-image explains all the facts as easily as does a cerebral seat. Hence the assumption of the latter by M. Binet and others is entirely superfluous. We now hope to show that it is not merely superfluous, but impossible.

Although we cannot so separate the fields of the two eyes as to determine to which of the two the image belongs, yet we can determine that it does not belong to both. This may be done by observing the different effect on the image in interfering successively with the field of each eye. If the location of the after-image is in the cerebral sensory center, we should naturally suppose

[1] Read at the Graduate Course in Psychology at Harvard College, January, 1889.

that through whichever eye it may be seen it will present the same characteristics. If it does not, this must be due to disturbances persisting in the right retina or optic nerve. But if this be once granted it will easily be seen that these persisting disturbances in reality alone account for the after-image and all its phenomena. It is curious to note that M. Binet also mentions the differences in action of the two eyes, but without once thinking of this most natural explanation. The principal differences observed are these :

1. If after obtaining the after-image the right eye alone is opened, the after-image is seen at once on the white ground. If, however, the left eye alone is opened, the image does not appear until the left field is so darkened as to allow the image on the right retina to assert its presence. If now at this point the right eye is again opened, it will be found that its after-image is still plainly visible.

2. After obtaining the after-image, close both eyes. If now the left eye be opened, the image is dimmed or blotted out ; and brightened when the eye is again shut. If the right eye alone is opened, the opposite effect is experienced ; when the eye is open the image is brighter, when shut it is dimmed.

3. Leave now both eyes open after the image is obtained on the right one only. Thrust before the left eye a finger, pencil, or other opaque object, and no change (except a possible increase in brilliancy) will be observable in the after-image. If, however, the object be placed before the right eye, the image disappears temporarily and does not reappear, if at all, until the right retina is newly fatigued by its new background.

4. The following corroborative observation is due to Mr. E. C. Sanford : "If I brand a strong after-image on my right retina and project it on a white surface, I see it green ; if I close the eye and project it on the dark field, I see it rose-colored. Now if I project it (as it seems) with the left eye (right eye closed, left open), I see it not green, but rose—exactly the tint in the closed eye, as I can see by quickly closing the left eye. I see it projected against the dark field—in other words, with the covered eye." In verifying this observation, I have found the following to be true in my own case : After obtaining the after-image from a red square of paper, if I project it on a white surface it appears *light* green ; if I close both eyes and cover them thickly in such a manner as to shut out all external light, the after-image appears a much *darker* green ; if, however, I merely close the eyes without covering them, so that considerable light can enter from without through the eyelids, the image then appears light rose, light blue, etc., according to circumstances. This affords a very conclusive test. I obtain a strong image with the right eye, then close and cover it, and open the left eye. Soon the left field darkens and the *dark green* after-image appears ; this gives place to the *rose-colored* image if the covering is removed from the right eye and the eye kept closed ; and this in turn to the *light green* if the right eye is opened. These three colors can be made to succeed one another indefinitely without in any way interfering with the open left eye, which alone, according to M. Binet, is the source of all the visual impressions present!

5. Retinal rivalry has been suggested by Prof. James as a test, and I also have found it reliable. After obtaining the after-image with the right eye, look at the background through colored pieces of glass, using both eyes simultaneously, but each looking through a

color different from that of the other; or by means of parallel vision, look with the right eye at a background of one color, with the left at one of another color. When the color of the left field is predominant, no after-image will be visible; when the right predominates, the image will be seen upon it.[1]

In the above experiments it will be seen that whatever be done to the left eye, the after-image suffers no change, except that of being brightened by diminishing the brilliancy of the left field, and dimmed or destroyed by increasing it. But whenever the right eye is interfered with in various ways, the image suffers corresponding modifications. These differences cannot be explained if we suppose the seat of the after-image to be cerebral. They are all easily explainable if the seat is retinal. M. Binet and others have made the great mistake of supposing that whatever they might see in the field of the left eye when it alone is open, is seen by that eye. That this is not necessarily so, and that in the phenomena presented by this experiment it is not possibly so, has been shown by the above facts. EDMUND B. DELABARRE.

Energy and Vision. S. P. LANGLEY. Am. Jour. of Science, 3d Series. Vol. XXXVI, p. 359.

Professor Langley has made a fresh determination of the brightness of the different portions of the spectrum. The investigation was made with great care, with a high sun, and errors from all possible sources were calculated and eliminated. The method chosen, after trial of others, was to determine how far away a screen carrying a portion of a table of logarithms had to be pushed in order for the figures to be just legible. The light from a slit, after passing through a collimating lens and a prism, was received on a silvered concave mirror, which formed a spectrum 90 mm. long in front of a second slit. Any color could be made to pass through this slit by setting a graduated circle; it then fell upon a black screen, through a hole in which, 1 cm. square, the table of logarithms was visible. The room was absolutely dark, and the position of the screen was got, by feeling notches, to within a centimeter. A variation of intensity of 225 times was had by the sliding screen alone. By changing the first slit and by introducing a photometer wheel, a variation a thousand times greater could be obtained. The selective absorption of silvered glass had been before determined by an ingenious method, and was now allowed for.

The results obtained are not easily compared with those of former observers (without making a graphical construction), for Langley's observations are taken at every .05 micron of wave-length, and those of Frauenhofer and Vierordt (the ones usually referred to) at Frauenhofer lines; but it seems plain that Professor Langley himself and his other three observers differ much more from each other

[1] It will be interesting to apply the following as yet untried test when a sufficiently sensitive hypnotic patient can be found who can obtain good after-images, but who has himself no theory as to "cerebral" or "retinal" seats. Let him obtain with the right eye a strong after-image, and then by suggestion paralyze completely the sight of that eye. If, then, no after-image is seen with the other eye open, it will prove that the cerebral center has nothing to do with the production of the image; if, however, the image is perceived, it will merely indicate that the paralysis of the right optic nerve has not been complete, and the experiment will have proved nothing.

than from Vierordt. The following table gives the results (for the non-normal spectrum) for Prof. Langley, another observer (*a*), and Vierordt, the two former reduced to 1000 for yellow, for the sake of the comparison:

	45	50	55	60	65	70
(*a*)	151	156	1000	996	208	16
V.	128	370	1000	780	128	22
L.	64	774	1000	141	13	2

These values of Vierordt are for points very near the wave-length at the head of the columns. Vierordt's method was to measure the amount of white light that had to be added to a given color to make the color undistinguishable. Capt. Abney's curve agrees closely with Prof. Langley's.

Prof. Langley has made use of his admirable determination of the distribution of energy in the solar spectrum to obtain the brightness per energy of the different colors. The mean of his three observers, exclusive of himself, is as follows:

Color,	Violet.	Blue.	Green.	Yellow.	Orange.	Red.	Crimson.
Wave-length,	40	47	53	58	60	65	75
Luminosity,	1.6	62,000	100,000	28,000	14,000	1200	1

That is to say, crimson light from the end of the spectrum has to have 100,000 times more energy than green in order to give enough light to enable us to read by it. The absolute work done by that crimson is .001 of an erg, and by the green .00000001 of an erg.

It is not quite plain why Prof. Langley should definitively set down the green as the brightest part of the spectrum, on the testimony of his three observers (he has drawn the curve for only two of them), when he himself, Capt. Abney, and all former observers, beginning with Newton (who says that yellow and orange affect the senses more strongly than all the rest of the prismatic colors together), have considered the brightest part to be in the yellow. Prof. Langley himself suggests that young eyes may be more effective towards the blue end of the spectrum than older ones. C. L. F.

Ueber die Unterschiedsempfindlichkeit des normalen Auges gegen Farbentöne im Spectrum. Dr. W. UHTHOFF. Archiv für Ophthalmologie, XXXIV, 4, pp. 1–15.

The sensitiveness of the eye to the change of color produced by a given change of wave-length has been investigated before by Aubert, and more recently by Mandelstamm and by Dobrowolsky. The last two made use of Helmholtz's ophthalmometer without the eye-piece; both plates were lighted up by monochromatic light, and one was then rotated until a difference of color was just perceptible. Dobrowolsky, by means of two Nicols with a quartz plate between them, caused both colors to be of equal intensity, a precaution which is particularly necessary near the ends of the spectrum. They found two points of maximum sensitiveness, one at *D* and one at *F*; the fraction of its wave-length by which a color had to be changed in order to seem changed was three times as great for green as for yellow or blue, and at the ends of the spectrum it was greater still. B. O. Peirce (*Am. Jour. of Science*, Oct. 1883) obtained a similar curve. König and Dieterici, by the method of mean errors and a different apparatus, got a curve which differs chiefly in rising very

abruptly at the two ends; beyond the wave-lengths μ.43 and μ.66 they affirm that absolutely no differences of color are perceptible, but only differences of brightness. Since the method of mean errors is considered by some to be objectionable, Uhthoff has repeated these experiments, using the same apparatus, but the method of just perceptible differences. His results resemble very closely those of the last two observers, but they differ somewhat at the ends. The difference is such as to leave it quite undecided whether it is caused by the difference of method or by the individual differences of the eyes experimented upon. Since the principal object of the investigation was to compare the methods, it is a pity that both methods could not have been applied by the same observer.

Peirce found the eye more sensitive to change in the yellow than in the blue; the reverse of that was found by all the other observers, but that is probably because his curve represents the mean of many different observers. He did not detect the changelessness of the two end-portions of the spectrum, but this, Uhthoff considers, is because he did not make the colors compared equally bright. The fact, if it is a fact, is of great importance for the theory of color-vision, and it would be well if it could be confirmed by other observers. Uhthoff says that the securing of equal brightness is less necessary in the middle of the spectrum than at the ends. This is strange, because, according to Langley's brightness-curves, the brightness is changing most rapidly on either side of the green. Can there be any significance in the fact that the change of brightness and the change of tone are both most rapid in the same part of the spectrum, namely, in the yellow and the blue? C. L. F.

Experimentelle Untersuchungen über die psychophysische Fundamentalformel in Bezug auf den Gesichtsinn. Dr. ARTHUR KÖNIG und Dr. EUGEN BRODHUN. Sitzungsber. der König. Preuss. Akademie der Wissensch. zu Berlin, 1888, pp. 15.

Doubting the applicability of the psychophysic law to light illuminations, these experimenters decided to find the differential threshold with various intensities, from the slightest barely perceptible illuminations up to intense brightness, almost glaring. The light was of six different spectral wave-lengths, 670$\mu\mu$, 575$\mu\mu$, 505$\mu\mu$, 470$\mu\mu$ and 430$\mu\mu$, these answering to the fundamental colors of their visions. König has normal color perception, but Brodhun is green blind. Their rather intricate apparatus and method of observation need not be detailed; the final problem in each observation consisted in adjusting one rectangular patch of spectral light so that it appeared just noticeably darker than another. The intensity of the illumination was varied for each wave-length from 1 upwards to 2, 5, 10, 20, 50, 100, 200, 500, 1000, 2000, 5000, 10,000, 20,000, 50,000, and 100,000, and downwards to .5, .2 and .1, the light 1 being the illumination seen by an eye looking through a diaphragm one square millimeter in aperture, at a surface coated with oxide of magnesium, the surface standing at a distance of one meter, and reflecting the light from a glowing platinum surface one-tenth square centimeter in area, standing parallel to it. Grouping all the wave-lengths together, it is found that between intensities 100,000 and 20 there is a very regular slight increase in sensibility, followed by an equally regular and slight decrease, the sensibility between inten-

sities 1000 to 20,000 being very nearly the same. The curve for this region would thus be a shallow trough. The average value of the sensibility between 100,000 and 20 is about 1/44. They also conclude (1) that the variations between the two observers are so slight as to show that the differences in their color distinctions do not affect the results; (2) that from the highest intensities down to 100 or 20, the wave-length does not affect the sensibility, the latter being a function of the illumination only; (3) that the three wave-lengths 670$\mu\mu$, 605$\mu\mu$ and 575$\mu\mu$ form one group, and 505$\mu\mu$, 470$\mu\mu$ and 430$\mu\mu$ form another group, the sensibility decreasing more rapidly with the latter than with the former. Again, the intensity of the light distinguishable from darkness was, from the longest to the shortest wave-lengths, .11, .011, .0055, .00035, .00013, .00014 intensities respectively. A very important conclusion that the authors draw is that with a brightness *subjectively* the same, the sensibility is independent of the wave-length; in other words, the same physical intensity in various colors does not appear the same psychically; and it is the latter that is the standard, not the former. J. J.

On some peculiarities of the phantom images formed by binocular combination of regular figures. Joseph Le Conte. Am. Jour. of Science, 3d series, XXXIV (1887).

The observations presented by Prof. Le Conte are four. (1) On looking downward at about 45° on a regular repeated pattern like that of an oil-cloth, and combining the patterns by crossing the eyes, the observer sees the phantom plane raised at the adjacent end, the elevation increasing as figures farther and farther apart are combined. The reason is that the rows of figures as they run forward from the observer slant inward (except the middle one) by mathematical perspective. As lines of figures with greater and greater slant are made by the crossing of the eyes to serve as the middle one of the field, they cross at greater and greater angles. Since this is what would actually be present if the observer were looking down a sloping plane, the image is involuntarily interpreted after that analogy. The individual figures appear elongated because referred to such a plane. The reverse effect is here as elsewhere to be obtained by combining the images with parallel lines of sight. (2) If a vertical plane is taken, on looking upward or downward the image inclines away in the direction of sight. (3) If the eyes are kept at the neutral point (about 7° above the horizontal for Prof. Le Conte himself), no such sloping is seen (if anything, the reverse curvature), but the plane falls away at the sides. If, however, the eyes are moved upward and downward, both curvatures appear. The sides are seen sloping because the points of the real plane to the right and left of the fixation point are represented on retinal points that belong to homonymous images, and are therefore interpreted as beyond the fixation point. (4) If the vertical surface is bent on its vertical diameter, like a half-open book seen from behind or before, the convex (or concave) effect is increased, as with the Le Conte Stephens stereoscope (Am. Jour. Sc. 3d series, XXIII, 297 ff.), but this increase is due to geometrical, not to retinal causes. The curvature of the plane in the third observation is a corollary of the circular form of the horopter in such a position of the eyes.

Neue Versuchungen an den Ohrenbogengängen. J. Breuer. Pflüger's Archiv, Bd. XLIV, p. 135, 1888.

Any doubt which may have remained among physiologists as to the effect of the semicircular canals upon the sense of direction must be completely set at rest by these admirable experiments of Breuer. The one thing which remained to be done—the separate excitation of the three canals by the electric current, and the production of the corresponding motions of the head under circumstances which excluded the possibility of any accompanying injury to the brain—has now been successfully accomplished. These experiments are, of course, very much superior in delicacy and conclusiveness to those in which the semicircular canals are cut. As little as possible of the ampullae is exposed, and the dove is kept in a state of stupor sufficient to prevent spontaneous motions. The strongest reactions—motions of the head through an angle of forty-five degrees—are got by the application of heat and cold. As Breuer showed in a former paper (1875), the motion, in the plane of a given canal, is in a different direction according as one end or the other of its ampulla is excited. When the electric current is used, the direction of motion changes with the direction of the current, and with the intermittent current no motion at all is obtained. The head of the dove may thus be made to move at pleasure in any one of six different ways, two in each of the three planes of the semicircular canals. That the current does not act directly upon the brain is proved in the following way: the point of the gold-tipped needle which forms the cathode is first inserted into the brain near the ear, and then the strength of the current is diminished until motion is no longer obtained in this way. The needle is then applied to the canals, and the same strength of current is here found to be sufficient to produce a marked reaction. It is not, of course, shown that the cerebellum is not concerned in the motion, but that the canals are the peripheral sense-organ for the centers in the cerebellum. Breuer's former experiments in mechanical stimulation of the canals he has repeated and confirmed. Motion of the head can be made to take place in one direction or the other according to the end of the canal from which the endolymph is sucked out by a scrap of blotting-paper; the direction of the motion is the same as that of the endolymph-stream.

It would seem that a more important rôle ought to be attributed to the semicircular canals in the derivation of the space-feeling than is usually done. It may also be conjectured that a fourth dimension in space will remain forever inconceivable to us until after we have developed a fourth semicircular canal. C. L. F.

Die Abhängigkeit zwischen Reiz und Empfindung. Dr. Julius Merkel. Philosophische Studien, IV, 4, pp. 541–596, and V, 2, pp. 245–292.

This elaborate paper recounts, with a mass of unnecessary details and a bewildering abundance of confusing tables, a series of experiments designed with great care, carried out with infinite patience, and directed to the solution of the most important problem of psychophysics. It would be impossible to notice here all the many points touched upon in this comprehensive study; only the conclusions reached, the methods used, and the inferences drawn from the results can be summarized. Ordinary observation would call

attention to the fact that sensation increases more slowly than stimulation; double as large a chorus does not give double the effect, and so on. But what the ratio between the two is remains to be determined. The logarithmic ratio is only one of a number. Moreover, the results by one method will not be directly translatable into another. Only by the agreement of several methods applied to the same problem can a conclusive result be reached. The main issue will be between the hypothesis claiming the distinctions between sensations to depend upon relative increments of stimulus, and that claiming that they depend upon absolute increments. The methods that Merkel employs are those of the just observable difference, the method of doubles, and the method of mean gradations. The first is used in a novel and improved form. It consists in asking the subject to adjust a stimulus so as to give a just appreciably greater sensory effect than a given stimulus; in other words, to record the difference at which a confidence nearing certainty is reached. The method of doubles consists in setting one stimulus so as to give a sensation twice as intense as a given sensation. The inference from the method is, that according to the one hypothesis the stimulus should be more than double, while according to the other it ought to be approximately correct. The third is a well-known one of Wundt's, and consists in adjusting a stimulus to give a sensation midway in intensity between a given pair of sensations. The argument is that if the "absolute" hypothesis holds the result would be the geometrical mean, while if the "relativity" hypothesis holds it would be the arithmetical mean. The two senses experimented upon were the sense of visual brightness and that of pressure with an admixture of the muscle-sense. For the former, three parallel dark chambers, each containing a lamp, were so arranged as to illuminate three disks with light of variable intensities. The adjustment was made by moving the lamps toward or away from the observer, and many precautions were taken to insure accuracy in the comparisons. The effect of contrast between the disks is the most disturbing factor in the experiments, and is compensated more or less completely by various devices. Calling the weakest illumination the apparatus could give 1, the range was from 1 to 4096.

The chief results are the following: (1) The method of the just observable difference shows Weber's law to be not valid between light intensities .5 to 64 (the sensibility increasing with the stimulus), but from 64 to 4096 the law is approximately correct; (2) the method of doubles shows that a stimulus is regarded as the double of another before it is really double, and this probably as the result of contrast; (3) the method of mean gradations shows a result much nearer the arithmetical than the geometrical mean; (4) the inference is that if two stimuli are to be distinguishable, the resulting sensations must bear a constant *ratio* to one another; (5) accepting this last hypothesis, one may say that if the stimulus .5 be regarded as completely converted into sensation, only .17 to .3 of the stimuli 64 to 4096 can be regarded as thus converted; (6) if the effect of contrast be eliminated in the method of doubles and of mean gradations, the results are in harmony with those of the just observable difference under the acceptance of the "relativity" hypothesis.

The pressure apparatus consisted, in essence, of a beam with a weight variable in size and in its position on the beam; the short end of the beam was provided with a cap upon which the finger

pressed down, thus supporting the weight attached to the other end. This downward pressure is evidently a muscular effort, even though the movement was only a few millimeters; and the resulting values seem to indicate a greater sensibility than the pure pressure-sense would give. By having three such beams, all three of the methods used with visual impressions could be applied to this kind of touch sensations. The pressures were varied from 1 gramme to 2000 grammes. The results expressed, as those with sensations of brightness, are as follows: (1) The sensibility increases as the stimulus increases up to about 200 grammes, and from there to 2000 grammes is quite constant. (2) The sensibility is finer (*a*) with successive than with simultaneous impressions; (*b*) when muscular sensibility is added to pressure sensations, than without the latter; (*c*) when the same finger is used for the various sensations than when different fingers are used; (*d*) when the surface in contact is small than when it is large—these points holding for all the methods of experimentation as well. (3) In the method of doubles, the ratios assigned as the double decrease as the stimuli increase. (4) By the method of mean gradations, the adjustments are much nearer the arithmetical than the geometrical mean. (5) On the basis of the relativity hypothesis, and assuming that with the sensation of 1 gramme all the stimulus is converted into sensation, then from 200 to 2000 grammes only .114 to .163 of it is thus converted; and a not very different result is obtainable from the other two methods when the effects of contrast are eliminated.

This research is thus in opposition to several of the accepted generalizations of psychophysics, and though some of this antagonism is more apparent than real, it will be a most delicate and difficult work to bring unity and harmony into this most perplexing field of experimental psychology. J. J.

Ueber den Rhythmus centraler Reize. Dr. R. v. Limbeck. Archiv für experimentelle Pathologie, Bd. XXV, H. 2.

The author has reopened the question of the rhythm of muscular contractions following central stimulation. Using induction shocks and recording the results graphically, he stimulated the cortex in dogs and rabbits and the cord in rabbits and frogs directly, and the cord in frogs, toads, rabbits and doves reflexly, stimulating the N. ischiadicus on one side so as to cause contractions on the other. In contradiction to the hitherto accepted view, he found that the central system did not send out motor impulses at a fixed rate, no matter how fast stimuli were sent into it, but that, within the limits of experiment, as many impulses were sent out as were received. His rates were for the cortex 6½–13 per sec., for the cord 5½–34, and for the same by reflex stimulation 4½–19½. Faster rates, when applied, gave smooth curves. Tracings of the spontaneous tetanus of strychnine poisoning showed a variable rate of central discharge.

Ein photometrischer Apparat zu psychophysischen Zwecken. A. Kirschmann. Philosophische Studien, V, 2, 1888, pp. 292–301.

Owing to the difficulties in the accurate observation of differences of sensations of brightness, such as contrast, differences in sensibility of neighboring parts of the retina, variations in accommoda-

tion, not to mention mechanical difficulties, Kirschmann devoted himself to securing a method of comparing luminosities free from such defects. He lays down two principles: (1) that the two surfaces to be compared must be equally distant from the eye of the observer, and if possible, in the same plane; (2) the two surfaces must be in contact. A tube 20 cm. in diameter, coated inside with lamp-black, and adjustable to a length of 60–100 cm., furnishes a dark chamber. In front of this a rotation-apparatus sets in motion a disk 21 cm. in diameter, just covering the opening of the tube. This disk has two quadrants white; and in the others a band of black or gray, and concentric with it an opening through which one sees into the tube. Both these are regulated in quantity by an adjustable portion of a disk attached over them. The object now is to make the black band equal in intensity to the black of the tube seen through the openings. If the opening is $a°$ wide and the black band $b°$, and the reflecting power of the black is called 1, of the white x, and of the tube 0, then $(360-a)x=b+(360-b)x$, or $x=\frac{b}{b-a}$. This is under the supposition that the tube reflects no light, which is not strictly true. If we call the slight light coming from the tube with an opening of $1°=k$, then the corrected formula is $(360-a)x+a^2k=b+(360-b)x$. And if in another case the settings are a_1 and b_1 degrees; then $(360-a_1)x+a_1^2k=b_1+(360-b_1)x$; from which the two unknown quantities k and x can be found. So slight, however, is this value of k (estimates make it less than 1/5700 or 1/6800 of the reflection from white cardboard), that it is not detectable in the general result. A comparison of a band of "*Pariser Schwartz*" with white cardboard by this method gave a ratio of 1 to 66.2, which agrees well with 1 to 68, found by Lehmann. Continual use seems to increase the power of reflection of black; making the ratio in one case 1 to 51.2. Similar measurements gave with lamplight a ratio of 1/60 (for black to white); with gas-light 1/58.2, and with diffused daylight 1/57.2. These differences are probably due to the impurity of the white of the cardboard. The following table of comparison with white cardboard of substances usually employed to produce black may be interesting:

	Lamplight.	Diffused daylight.
Paris black,	1/60	1/57.2
Indian ink,	1/23.6	1/20.2
Indigo,	1/26.8	1/27
Graphite (Faber BB),	1/8.6	1/8.9
" (" B),	1/6.2	1/5.9

This apparatus seems to be satisfactory and commends itself to various uses. J. J.

IV.—ABNORMAL.

De la Paralysie Générale d'origine traumatique. B. Ball. L'Encéphale, July, 1888.

Paralysie Générale d'origine traumatique. L. F. Arnaud. L'Encéphale, November, 1888.

Prof. Ball reports three interesting cases of general paralysis of traumatic origin, and such cases are so rare as to merit special comment. The first was a postal employé who was thrown against the

side of a wagon. His hereditary antecedents and personal habits were irreproachable, and the disease was clearly traceable to the traumatism. The injury was received February 28, 1887, and death occurred from exhaustion, September 16, 1887, giving a period much under the usual duration. Dr. Arnaud's article is an account of the autopsy, which fully confirmed the diagnosis.

The second case was that of a mechanic who was struck by a ball of globular lightning during a severe thunderstorm. In this case, as in so many where the disease is dated from a fixed time, the traumatism appears to have been merely the shock necessary to light up into activity the disease which had existed in a dormant state for some months.

The third case is the most interesting of all; the patient received a blow on the left forearm, injuring the ulnar nerve, and followed by muscular atrophy. Mental troubles came on after the accident, and a well marked case of general paralysis developed. Here the general paralysis was directly due to the traumatism, and the case is of great interest from the exactness with which it was possible to show the origin of the disease. W. N.

Ueber das melancholische Anfangsstadium der Geistesstörungen. Ludwig Schirmeyer. Inaug. Dis., Strassburg, 1886.

The author tabulates statistics of 290 cases (melancholia 98, mania 69, paranoia (*Verrücktheit*) 88, feeble-mindedness (*Schwachsinn*) 20, distraction (*Verwirrtheit*) 15) in order to determine the frequency of a beginning in melancholia. Counting in the melancholiacs, less than one-half of all showed a depressed first stage; throwing out the melancholiacs (except some cases not fully observed), the proportion is only about one-fourth. Taken separately the proportions were, mania about one-third (or excluding certain cases for cause, about one-fourth), paranoia a little more than one-seventh, feeble-mindedness one-fourth, distraction over one-third. The melancholiac terminal stage, defended by some, was found in but few cases. Insanities are often preceded by conditions of ill-feeling, but this is by no means the melancholia of the alienist. The author's figures bring him into agreement with Witkowski, and into opposition with Arndt. The statistics are preceded by an extended summary of previous opinions.

Ueber Bewusstseinsstörungen und deren Beziehungen zur Verrücktheit und Dementia. Dr. J. Orschansky. Archiv f. Psychiatrie, Bd. XX, H. 2.

The author points out in a group of psychic conditions, variously named by various authors (*Verwirrtheit*, Wille; *la démence*, Esquirol; *acuter hallucinatorischer Wahnsinn*, Meynert; *acute primäre Verrücktheit*, Westphal; *acuter sensueller Wahnsinn*, Schüle; dreamy-condition of different authors) a common element, to wit, a deep obscuration of consciousness, or an "ataxy in the psycho-physic sphere." In a typical case the patient is cloudy in his conception of himself; the bounds of his *ego* and the non-*ego* are obscured; his notions of time and space are uncertain; memory images and acquired associations are weakened or lost; if at times he is able to recognize his surroundings, he is not able to connect the impression of them with similar impressions before received. As a consequence,

right perception and logical framing of ideas are out of the question. In the fields of volition and emotion the obscuration is as great. With its depth the expression of it changes from a simple dazed condition to one resembling coma. Upon this simple background may appear illusions, hallucinations, and delusions to such a degree as to color the whole; and since the condition frequently enters only as a stage in a complicated psychosis, it is apt to be still further overlaid by the residua of delusions from earlier stages; whence its confusion with primary dementia and paranoia, the two extremes between which it varies. A typical case begins and ends with periods of disturbance of consciousness passing through a delirious stage between the two, and having a favorable prognosis in proportion as the obscuration of consciousness is deep. For the author, as for Wundt, consciousness has a functional side, and it is by no means simple. It involves the regulative ideas of the *ego*, cause, time and space, etc., and by its complexity makes possible a "change of consciousness as a whole, which to a certain extent is independent of the quantitative and qualitative change of the elements of consciousness, as also of the psychic function, *i. e.* of the framing and working over of ideas."

Klinische Beiträge zur Kenntniss der generellen Gedächtnissschwäche. Dr. C. S. Freund. Archiv f. Psychiatrie, Bd. XX, H. 2.

The author has observed two cases of pathological weakness of memory. Both were women, one 52, the other 65; both had suffered severe nervous troubles from alcoholic excess, leading up to psychical disturbances, after which the chief remaining symptom was the weakness of memory, complicated in one case by senile dementia. In both the sensorium was clear of hallucinations and sense illusions, apperception little affected, and untroubled by delusions; bodily disease was absent, except in one, whose trouble was tabetic. The memory of things in early life (before 30 in the first, before 20 in the other) was much better than for recent events. The first could not remember her own bed, nor the day of the week, nor, after a few hours, whether or not she had dined. She forgot repeatedly the name and use of a stethoscope that was shown to her and explained; she could only repeat phrases of some length when spoken slowly and distinctly and perhaps repeated. Similarly with tunes, and the first tune of several seemed best remembered. She could multiply numbers, but forgot the examples; she could, however, remember a number over a period of conversation about other things. She showed also illusions of memory. Her power to write was not affected. In the second case, however, this was an interesting feature. In spontaneous writing she repeated words in whole or in part, together with parts of words to follow, before getting the latter written correctly. She had difficulty in recalling the capital letters, though what was at the instant forgotten often returned later. Writing from a copy became almost free of errors when she spelled the words aloud as she wrote. Writing from dictation was worst of all. In the dictated alphabet errors seemed to occur in proportion to speed. The patient could recognize errors, and the disturbances varied with her power of memory. That sufficed for her to spell polysyllabic words orally, but not to write them, and in spelling she was apt to add letters after completing the word. In other particulars she resembled the first case.

Ueber epileptische Amnesie. HERMANN HELMING. Inaug. Dis. Halle, 1883.

Dr. Helming has studied, with special reference to memory, a case of epileptic insanity. The patient had epileptic spasms occurring periodically, showed a suspicious and violent character during intervals of sanity, and in periods of insanity manifested characteristic symptoms of epilepsy, having sense hallucinations, great ideas, religious delirium and the like. Yet, generally, for some time after attacks of insanity, the patient remembered all that had occurred, except what happened during the convulsions. Also in attacks of insanity he remembered the events of previous attacks. Hence the author concludes that defect of memory, which has been deemed a chief characteristic of epileptic insanity, cannot be relied upon. The ability to recollect what has occurred during an attack does not exclude the possibility of epilepsy. Instead of depending upon a single symptom, more stress should be laid upon the general character of the attack and the epileptic nature of the patient. This thesis contains a brief summary of theories of epileptic insanity, and to it is appended the usual bibliography. W. H. B.

On the Dissolution of the Functions of the Nervous System in Insanity, with a Suggestion for a New Basis of Classification. J. MACPHERSON. Am. Jour. Insanity, January, 1889.

Following Hughlings Jackson, the author looks upon the insanities of maturity as dissolutive. In general this dissolution is shown chiefly in one of the great fields of mental function, emotion, intellect or will, though more or less in all. Dissolution in the first results in melancholia and mania, in the second in the delusional insanities, in the third in insanities of the moral and impulsive kinds, and, since the power of origination depends on will, in dementia.

Alcoholic Heredity. Dr. F. LEUTZ, Medical Director Government Insane Asylum, at Tournai, Belgium. Quarterly Journal of Inebriety, April, 1888.

Dr. Leutz adds nothing new to our knowledge of alcoholic heredity, and his short paper is simply a review of the opinions brought forward by others, but the subject is of such universal importance that it may not be improper to note the two forms that alcoholic heredity assumes. The first is homologous heredity, or that of similitude; second, the heredity of transformation, or eccentric heredity. In the first form the progenitor gives to the descendant his tendency to alcohol, or symptoms of his alcoholism; in the second form, the alcoholized mental state of the progenitor becomes transformed into varied nervous disorders. No one denies the direct heredity, but there is but little agreement about the frequency of its transmission. The multifarious forms in which the second class appears, in irritability, instability, and a vicious moral disposition, need not be dwelt upon. W. N.

Proceedings of the Congress of the National Prison Association of the United States, held at Boston, 1888. Edited by the Secretary, FREDERICK HOWARD WINES, Springfield, Ill.

While the Proceedings are naturally and by right mostly taken up with questions of administration and of the improvement of

the criminal classes and the possibilities of their reform, there is one subject dealt with here that is of interest to the student of morbid psychology, the report on *The Registration of Criminals* (p. 73). "The new method of identifying prisoners, introduced into France by M. Alphonse Bertillon, and which is now successfully practiced not only in all the French prisons, but also in Russia, Japan, Spain, Italy, and some parts of Germany, has, within the past year, made quite a headway in the United States. It consists in the exact measurement of the prisoner on his arrival at the jail or prison. His height, the length and width of his head, the left foot, the outstretched arms, the trunk of the person seated, the four fingers of the left hand, the left arm, and the length of the ear are measured; and the color of the eyes and any particular marks are noted down; and a photograph of the prisoner is taken, both in profile and full-face view. It will be noticed that all the measurements mentioned are those of the bone dimensions of the human body after the body has attained its mature growth." The measurements thus obtained are catalogued and systematized in such a manner that it is possible to identify immediately, by reference to a central office, any person whose measurements have been recorded. The psychological interest of this new departure is that it promises some light on the existence or non-existence of the so-called criminal type. If such measurements are carried out systematically over a series of years and are then submitted to rigid investigation, the means would seem to be at hand for settling some of the questions raised by Lombroso and others of the new school of criminologists. The practical value of the method for gaining the result sought, the identification of criminals, has been abundantly proved in France, where about one hundred thousand cases are filed, and where they are used every day in furnishing information to the courts; no mistake has as yet occurred. W. N.

Verbrechensverübung im Traumwandeln. Dr. Hermann Ortloff. Gerichtlich-medizinische Fälle und Abhandlungen, Heft II, 1888, pp. 35–64.

The author gives in readable fashion a case of alleged arson that came before him for investigation, in which the question of crime in sleep-walking held an important place. A fire broke out at night in a farmer's house, destroying a large part of its contents, including valuables belonging to his wife. Suspicion fastened on the wife herself, chiefly on the testimony of the maid-servant, who asserted that her mistress was eccentric, went about the house at night, and had brought fagots into the house just before the discovery of the fire. The place where the fire started made it quickly destructive to the property of the mistress, and for other reasons it seemed clear that if she set the fire she did so without consciousness. The assertions of the servant were, however, denied, and she herself was not above suspicion. The age of the mistress, 37, the slight nature of the nervous trouble from which she suffered, and the planfulness of the act were against the sleep-walking theory. The case was dismissed for lack of evidence, but the question remains, was it or was it not a sleep-walking crime?

V.—MISCELLANEOUS.

Psychologie de l'Attention. TH. RIBOT. Paris, 1889, pp. 180. F. Alcan.

The convenience of M. Ribot's compilations will make welcome all contributions from his pen. Like his former works, the present monograph opens out a comparatively novel topic and treats it suggestively. Attention is a concentration of the mind, a monoideism. The typical movement of thought is a polyideism; a lighting on one thought, dwelling upon it slightly, then flitting to another, and so on. Change, movement, is the normal law. Attention, furthermore, is of two kinds, spontaneous and voluntary. The latter is the one most commonly treated, but is theoretically less important. The more primitive is the former, in which normal vital interest guides the attention. We see this in animals attracted by all that is connected with food; in children staring at the brightest color and listening to the loudest sound. In voluntary attention we are guided by what experience has proved to be useful. We substitute an artificial end for the natural attractiveness, until the power of holding the attention becomes second nature. This is the power that is educationally so useful; the absence of which is typical in the shiftlessness, the instability of the hereditary criminal, and its presence typical in the devoted student absorbed beyond all interruptions in his chosen work. To the description and detailed exposition of each of these psychologic processes M. Ribot devotes a chapter. The third chapter is devoted to the morbid states of the attention. A variety of forms occur. There is the constant dwelling upon trifles, the fixed ideas, the reasoning mania, where one cannot get away from attending to one narrow trivial train of thought; there is the lack of concentrative power seen in idiocy, in the incoherence of mania, in dementia, and in milder forms, from the influence of drugs or a headache. Perhaps the apex of concentration is seen in the trance states of hypnotism, where the mind seems a blank, except the single point upon which the suggestion has been given. The religious ecstasies are of a similar kind, and the case of St. Theresa distinguishing seven stages of ecstasy is given as a type. The mechanism of attention is motor. It is initiative action. Whoever cannot control his muscles cannot control his attention. The motor accompaniments are not merely accessory; they form an essential part of the process. In concluding, M. Ribot completes his valuable survey by noticing the physical antecedents of good attention, regularity, a healthy nervous system, and the like. Brief as the monograph is, it offers about as complete and methodical a treatise on attention as we possess. J. J.

Recherches sur la fatigue mentale. FR. GALTON. Revue Scientifique, Jan. 26, 1889.

In this paper Mr. Galton gives a résumé of 116 replies sent in by members of an English teachers' organization to six questions of his on the signs and extent of mental fatigue. The answers bring together a considerable list of such signs, and furnish an excellent basis for subjective and objective observation. Of special interest is a case in which fatigue caused temporary fits of color-blindness. As tests for fatigue the following are suggested: the length of time

that one can work carefully at a long piece of work; the promptness and certainty of memory for common things; arithmetical problems demanding "common sense"; the reaction time from hand to hand, which, of course, could only be employed with a class. Two conclusions are reached from the collation of the answers: one, that the reason mental fatigue is more wearing than physical is because it interferes with sleep; and second, that those most likely to overwork are those that work by themselves, especially those preparing for professions under unfavorable circumstances. The majority of students do not overwork; those that do are those that feel their power and are ambitious to succeed.

Erklärung der Sinnestäuchungen bei Gesunden und bei Kranken. Prof. J. I. HOPPE. Vierte Auflage. Würzburg, 1888, pp. 306.

This is largely a collection of cases of illusions and hallucinations from the psychiatric literature, largely supplemented by personal observation, with which the author seems to have busied himself very constantly. All states seem to have their appropriate hallucinations, though ordinarily we should not classify them as such. The illusions of sight naturally form the largest section of the work, and the contents of that may be cited as typical of the treatment. After distinguishing between hallucinations and illusions, the first section discusses the subjective phenomena of the retina; the second section gives the observations as drawn from what one sees when closing the eyes, and an account of the literature on this point. The third section gives explanations, and the fourth deals with illusions in particular. Hearing, touch, taste and smell are treated in a similar manner. The treatise is thus not an encyclopaedia of illusions, nor a well-developed theoretical exposition like the book of Sully, but rather a collection of cases and facts from which each one can elaborate his own views. J. J.

Consciousness of Lost Limbs. WILLIAM JAMES. Proc. Amer. Soc. Psychical Research, Vol. I, No. 3, Dec. 1887.

Circulars of enquiries were sent out and the answers compared. About three-fourths of the patients feel the limb after it is lost. Some had only a temporary and rapidly fading consciousness of the lost member. In one-third of the cases the toes may be moved at will. As faradization will restore the sensation of the lost limb, the loss of consciousness is due to habitual inattention. Since the real sensations of the limb if present are gone, hallucinations that may arise with reference to it are allowed free scope, and, like rudimentary organs, have also a tendency to vary. J. N.

The Psychology of Deception. JOSEPH JASTROW, Ph.D. Popular Sc. Monthly, Dec. 1888.

After an introductory exposition of the importance of the psychic addition to sensation in the process of perception, the author illustrates fully and explains three groups of false perceptions. The first is of the senses, a spoon seems bent in a glass of water; this is easily corrected by experience. The second is due to ignorance of the technical matters involved and of the lures that carry the attention this way and that; on this depend the tricks of conjurors. The third are those of expectant attention, mental conta-

gion and the like, in which the chief deceiver is the observer's own self; here belong the cases of witchcraft and the cheap miracles of modern séances. For this kind of false perception the only help is prevention, the cultivation of sound and independent habits of mind, and a knowledge of when consciousness is likely to be an unreliable witness.

Statistische Untersuchungen über Träume und Schlaf. FRIEDRICH HEERWAGEN. Wundt's Philos. Studien, V, 2.

In order to answer the question, "Do people who dream much have lighter or sounder sleep than the average," a series of questions were asked of 142 women, 151 students, and 113 men of various occupations. Of these, 99 dream all night long, 133 often and 153 seldom, the remainder never; 216 have vivid and 175 not vivid dreams; 194 can easily recall the dreams, and 203 have difficulty in recalling them. 10.30 is the average time of retiring, and 0–3 hours are the extremes of the time needed to fall asleep. 261 sleep all night uninterruptedly and 143 have waking spells. 166 are heavy sleepers, 202 are light. 103 can sleep at will during the day (after-dinner naps not counted); 182 find the forenoon best for mental labor, 133 the evening, the balance are indifferent. 132 are of sanguine temperament, 70 are choleric, and 74 are phlegmatic; 20 are melancholic, the others are mixed.

These statistics serve to give an idea of the nature of the questions asked. The results were tabulated and the following laws deduced: Sleep is soundest in childhood, and becomes increasingly lighter with age. Dreaming reaches its maximum intensity and frequency at an age of 20–25 years, the increase from childhood and the decrease toward senescence being gradual.

The following factors are in close relation and vary together: Vividness of dreams, frequency of dreaming, lightness of sleep, power of recall, length of sleep or the number of hours needed, nervous or anti-phlegmatic temperament. Women dream more than men, and students stand as a class between other men and the women. Married women dream less than the unmarried. The deeper the sleep the easier it is to do mental work in the morning. A medium length of time required for falling asleep is connected with frequent dreams and light sleep. Men sleep most soundly and women least so. Those whose sleep is most interrupted also sleep most by day. Some who on rising feel weary rapidly regain vigor. There are, however, individual exceptions to all the above rules, as when frequent dreaming is connected with deep sleep; the stimulus that wakes a person up is provocation of a dream in this case. We must also remember that the curve of depth of sleep is lowest in the morning when dreaming is most frequent.

J. N.

Earthquake Effects, Emotional and Moral. JOHN MILNE. Trans. Seismological Soc. of Japan, Vol. XI.

Note on the Effects produced by Earthquakes upon the Lower Animals. JOHN MILNE. Trans. Seismological Soc. of Japan, Vol. XII.

In the first paper the author adds to a few general observations on the physical and moral effects of earthquakes, a number of extracts from miscellaneous sources, showing the state of mind into which men are thrown by such calamities, and finally traces some possible

effects (but by no means all of them certain) upon national character. The most tangible are the creation of earthquake gods or monsters, and the introduction of their cults. In the second paper the question of animal foreknowledge of such phenomena is discussed. When the animal's agitation precedes the shock by some time it has nothing to do with it, but when by only a half minute or less the animal may feel the fainter ripples that run before the main waves in heavy earthquakes. Under favorable circumstances the shock may thus be anticipated as much as 10 or 15 seconds by human observers.

Vollständiges Lehrbuch der Gedächtniskunst. ADOLF KÜHNE. Osterwieck, Harz.

Die Gedächtniskunst im Dienste des Lehrenden und Lernenden. J. FIEWEGER. Breslau, 1888.

A. Kühne bases his art upon the laws of association and the principle that the learner should proceed from the known to the unknown. He approves what Kant called the *judicióse* method in mnemonics, and maintains that without the help of the understanding the memory is always weak and untrustworthy. He adopts a figure alphabet, associates the fact to be remembered with the mnemonic word after the fashion of some of the older mnemonic teachers, and applies his principles chiefly to the learning of dates, tables and the like.

J. Fieweger gives many illustrations of the application of the older principles of the mnemonic art in learning difficult tasks. Such books are chiefly interesting psychologically as showing the cumbrous devices that some people find helpful for forcing the attention to disagreeable things. W. H. B.

Beiträge zur Kenntniss der Physiologie und Biologie der Protozoen. Bericht der naturforschenden Gesellschaft zu Freiburg, Vol. I, 1886.

This paper is of great psychological as well as of biological interest. It was found that when these unicellular organisms (*Stentor*, *Amoeba*, etc.) were artificially cut up, that the part cut off was restored to its perfect form by regeneration, no matter what was the portion of the cell that was lost; so that when the cell was divided two or more times into approximately equal parts, each part restored itself to the perfect animal. It was found that no portion would thus restore itself unless it possessed a fragment of the nucleus (which in Stentor is a long-headed filament). The conclusion is plain, viz. that any portion of the nucleus possesses protoplasm which has the complete characters of the entire being, and which controls the nutrition of the extra-nuclear protoplasm. That is, the molecules of the nucleus are *idioplasm*, and each is like the others, and in itself is capable of restoring the entire cell. These idioplasm molecules may of course be different from chemical molecules; experiments throw no light on that. Partial or incomplete division leads to the formation of individuals that remain united by the part left uncut. Studies of the spontaneous fission of these forms revealed that the daughter-cells of one form divided together, although separated into different watch-crystals and in different sorts of water. This same synchronism of reproductive activity of protoplasm closely related, was observed in the artificial sections, even when these differed greatly as to size. J. N.

NOTES.

The time of mental processes in the insane has been measured by Buccola, Tschisch and others, and recently again in Bechterew's laboratory by Marie Walitzkaja (*Wjestnik psichiatrii i nevropatologii*, 1888, VI). This lady worked upon seven insane subjects (3 general paralytics, 3 maniacs in various states of remission, one of which passed on into the excited stage while the experiments were in progress, and 1 paranoiac in a stage of remission), and for comparison, on five persons of sound mind, taking in all about 18,000 records. For the sane the figures are about normal; those for the insane show differences. In simply reacting to sound, the general paralytics alone were slow; most required more than 0.3 sec. and some nearly 0.5 against 0.168–0.207 sec. for the normal. In reacting to one of two sounds all the insane were slower, the slowest being those in general paralysis and in maniacal exaltation, those in remission approaching the normal. In reacting with one hand to one sound and with the other hand to the other, the delay was yet more marked; the normal time was 0.351–0.406 sec., that for those in general paralysis 0.707–0.943 sec., and that of a subject in maniacal exaltation 1.085. The association time was slower than normal for the general paralytics and for one of the others (one with a melancholic tendency), but quicker for the maniacal condition, 0.194–0.322 sec. against 0.664–0.716 for the normal.

Dr. Chas. A. Oliver some time since examined the eyes of twenty young adult male imbeciles (*Transactions Amer. Ophthal. Soc.* 1887) with a view to establishing what the normal eye should be when not used for near and careful work. He found them sound and as healthy in appearance as those of babies; and therefore concludes that such things as "insufficiency of the interni, dirty-red gray appearance of the optic disk, irregularity of physiological excavation, non-visibility of the superior and inferior portions of the scleral ring, absorbing conuses in all their varieties, increase in density and thickness of the retinal fibers, opacities of vascular lymph-sheaths, disturbed states of the choroid, and gross errors in astigmatism with changes in indices of refraction,"—things frequently found in used eyes, are the result of use and abuse.

Dr. C. Pelman, the director of the provincial institution for the insane at Grafenberg, near Düsseldorf, takes up, in a recent brochure (*Nervösität und Erziehung*, Bonn, 1888, 41 pp. 8vo), the question of educational hygiene. In assuming that nervousness is a distinguishing characteristic of the present generation, he offers no statistical proof, but appeals to the universal conviction. He understands by "nervousness," "conditions of a diseased excitability of the nervous system, which may also be well designated by the expression 'irritable weakness.'" Under the influence of a series of injuries, the nervous system loses its primitive power and sta-

bility. As a consequence, external impressions, which in a normal state would not be felt, or would be felt only in a moderate degree, excite abnormally great sensations which stand in no correct relations with the outer impressions. The draft thus made on the nervous resources is followed by greater weakness, and this weakness is nervousness. This form of weakness is the concomitant of increasing culture. Travellers agree in asserting that primitive peoples are seldom thus afflicted. While the American negroes were in the condition of slavery they were seldom nervous, but now mental diseases are common among them. America, according to Dr. Pelman, is "the promised land of nervousness." This is not, however, because of the superiority of American culture, but on account of the recklessness in the use and economy of nervous force which prevails there. It was in America that the worst forms of nervousness first appeared, especially neurasthenia. The cause of nervousness is found by our author in over-tension brought about by many particular circumstances, which may in general be summed up in "fast living," the prominent feature of city life, even in its accepted virtuous phases, and which may in turn be defined as continuous excitation without repose. The first and principal symptom in which nervousness expresses itself is the want of that power of resistance which every sound man is able to exercise against external encroachments. Constancy and firmness of character are lost. The man becomes too feeble to react against his environment, and becomes its slave: In extreme cases this loss of character extends to self-destruction, through inability to cope with the actual conditions of life. Less desperate cases seek the restoration of consciously lost power in the use of alcohol, opium and other drugs. Where restraining motives operate to preserve one from these, ideal equivalents are often sought. "To take in the hand a romance of Walter Scott's," says Dr. Pelman, "would be as tedious as comical. That is all so home-bred, so healthy, and if the end of the story is that they catch each other, wherefore so many pages and circumstances! How differently the modern romance-writers apprehend life! There one can see how it actually is, and a pistol-shot is a different solution from an ordinary marriage." After a brief discussion of heredity, in which he shows how nervousness leads to drunkenness, and the drunkenness of parents to the nervousness of children, the author touches the subject of education. "On education," he says, "devolves the double duty to make good the loss which birth has caused," or, in a word, to undo the evil of heredity. The author finds it impossible, as a physician, to approve of the modern education, yet he realizes the danger of merely one-sided criticism of a system which is, in its essence, so important to public welfare. He ventures to assert that "the child works *too early*, works *too much*, and works *badly*, that is, under unfavorable hygienic conditions." These criticisms are directed against the present requirements in Prussia, which are undoubtedly more severe than those in the United States, but would certainly apply to school exactions in many parts of our own country. The author pleads for moderation and diversity in the brain-work of boys and girls. He enlarges upon the evils of nervous over-strain for women. According to Candolle, the number of girls devoted to teaching, that have entered the Swiss asylums for the insane, is enormous. In England, the proportion of women teachers in similar institutions, according to Shaftesbury, in 1882, was astonishing, being 145

women to only 38 men. The proportion in Germany, in 1879, was not quite so great, being 186 women to 131 men. There entered in the course of the year 44 men and 56 women, showing a ratio of 7:10. Dr. Pelman adds: "What we have said concerning the large share which female teachers have in nervous diseases and weaknesses is true in a still higher degree of female office-clerks, bookkeepers, saleswomen, and post and telegraph assistants, and I am firmly convinced that there are few in these classes who do not bear through life their share of nervousness." As remedies the author names no medicaments, but urges a sacred regard for sleep, exercise, fresh air, the selection of sound partners in marriage, and avoidance of exciting competition. As a whole, the treatment is more suggestive of thought upon a wide-reaching and most important phenomenon in modern life, than satisfactory as an exact and specific solution of the problems which it raises. D. J. H.

Statistics of 1985 epileptic seizures in Dr. Féré's wards in the Bicêtre Hospital (*Compt. rend., Société de Biologie*, Nov. 17, 1888) show that nearly two-thirds of them fell between 8 P. M. and 8 A. M., and were grouped especially about 9 P. M. and 3–5 A. M. On the theory that dreams are most frequent in the first and last hours of sleep, a connection of the two suggests itself, and the questions are started whether both have a common cause, and whether the dreams influence the convulsions.

Bourneville and Sollier have contributed to the question of the proportion of physical abnormalities in idiots and epileptics the statistics of an examination of the genitals of 728 persons of these classes (apparently all males) in the Bicêtre Asylum (*Progr. Méd.* 1888, No. 7). Some sort of malformation of those organs was found in 262 persons, a number much in excess of the normal proportion. With epileptics whose attacks began after the first years of life, the proportion was much less than with simple idiots, and impotence was less frequent. On the other hand, varicocele was rarely present in the non-epileptic cases. Idiots that become such in very early life show greater malformations of all sorts than those whose defect dates from puberty or later. In so far as malformation results in impotence, it reduces hereditary disease of these kinds.

Of 10 persons caught feigning among 200 examined, Dr. J. Fritsch reports (*Jahrbücher f. Psychiatrie*, VIII, 1–2) only 2 as of completely sound neuro-psychic organization. Three more came short of actual alienation, but were neuropathic and excitable; five were fairly to be called insane. Seven were under arrest for crime against property, and two more had been. Criminals of this kind are fitted for feigning by their characteristic skill in lying, exaggeration and dissimulation.

A. Marro (*Annali di Freniatria*, I, 1888) has been led by observations on 22 paralytics to regard peptonuria as a characteristic accompaniment of progressive paralysis. In doubtful cases he would risk diagnosis on that symptom. It has been before observed in the insane, attending grave disturbances of nutrition, latent suppuration, etc.

In some diseased states of the nervous system, stimulation of a member on one side is felt in the corresponding member on the other. This phenomenon, called "allochiria" by Obersteiner and observed by him in tabetics and one hysteric, has been observed as a transient symptom by Dr. A. Huber (*Münch. med. Woch.* 1888, Nos. 34 and 35), in a case of multiple sclerosis of the brain and cord.

Five cases where spinal trouble gave a sharp boundary between sensitive and anaesthesic areas have been used by Prof. Eichhorst (*Zeitschr. f. klin. Medicin*, XIV, 5–6) for the discovery of the surface distribution of the nerves from the spinal roots. He found, contrary to common assumption, that the areas are not bounded by straight lines, but by curves, of which there were among his patients two types: one which showed on each side of the body an elevation between the vertebral and scapular lines, between the scapular and mammillary lines, and on the sternum; and another which showed elevations near the vertebral column, between the scapular and mammillary lines, and near the sternum, the last, however, not being united as before with the corresponding elevation on the other side.

An interesting case of what might be called motor musical aphasia has been added to one previously observed by Prof. Kast (XIII Wandervers. der Südwestdeutschen Neurologen und Irrenärzte, *Archiv f. Psychiatrie*, Bd. XX, H. 2). The earlier case was a talented member of a country singing society, who suffered right hemiplegia and motor aphasia from a wound of suicidal intent. The new one is that of a cultivated musical amateur (solo singer and violinist), a merchant of 45 years, who showed motor aphasia with disturbances of writing after a couple of apoplectic attacks. In both cases the perceptive musical faculty was preserved, and both recognized their failures in musical execution. The second lost his instrumental as well as his vocal music, though his ability to read notes was retained, his mental image of the tune was perfect, and the rhythm of his attempts precise. His speaking, which in the year and more since the attack had received the most practice, had returned much more completely than his musical powers, his whistling and singing being better than his violin playing.

Prof. Kirn, while admitting that there are criminals by inheritance, takes position (in v. Holtzendorff and v. Jagemann's *Handbuch des Gefängnisswesen*) against theories that would thus account for crime in general; there are those in penal institutions in whom psychology and anthropology can find no abnormalities. For the degenerate that are not fully irresponsible he approves imprisonment, but in institutions where they can be treated individually.

The *Journal of Mental Science* for January, 1889, contains a letter written to a well-known member of the Brit. Medico-Psychological Assoc. by a young woman who reformed after practicing laudanum drinking to the extent of four ounces daily. The letter is interesting in giving an inside view of the origin and mental and moral effects of the habit and of its cure.

Prof. Forel gives it as his experience (XIII Wandervers. der Südwestdeutschen Neurologen und Irrenärzte, *Archiv f. Psychiatrie*, Bd. XX, H. 2) that a short and complete stop in the use of liquor, even in delirium tremens, is harmless, from 4 to 5 days being generally sufficient for dishabituation. Nourishing food is essential, and should be given by force if necessary. Of 24 cases treated since September, 1886, at the insane asylum of Burghölzli, not a promising class of patients, 10 had so far (June, 1888) remained abstinent, 5 had relapsed, 2 were doubtful, 6 of unknown residence, and 1 chronically insane. Hypnotism was an important help with very suggestible subjects, but for permanent effect had to be united with entrance into a total-abstinence society.

The following circular has been addressed to medical microscopists:

"In behalf of 'The American Association for the Study and Cure of Inebriety,' the sum of one hundred dollars is offered by Dr. L. D. Mason, vice-president of the society, for the best original essay on 'The Pathological Lesions of Chronic Alcoholism Capable of Microscopic Demonstration.' The essay is to be accompanied by carefully prepared microscopic slides, which are to demonstrate clearly and satisfactorily the pathological conditions which the essay considers. Conclusions resulting from experiments on animals will be admissible. Accurate drawings or micro-photographs of the slides are desired. The essay, microscopic slides, drawings or micro-photographs are to be marked with a private motto or legend and sent to the chairman of the committee on or before October 1, 1890. The object of the essay will be to demonstrate: *First*, Are there pathological lesions due to chronic alcoholism? *Secondly*, Are these lesions peculiar or not to chronic alcoholism? The microscopic specimens should be accompanied by an authentic alcoholic history, and other complications, as syphilis, should be excluded. The successful author will be promptly notified of his success, and asked to read and demonstrate his essay personally or by proxy, at a regular or special meeting of the Medical Microscopical Society of Brooklyn. The essay will then be published in the ensuing number of *The Journal of Inebriety* (T. D. Crothers, Hartford, Conn.), as the prize essay, and then returned to the author for further publication or such use as he may desire. The following gentlemen have consented to act as a committee: Chairman, W. H. Bates, M. D., F. R. M. S. Lond., Eng. (President Med. Microscopical Soc., Brooklyn), 175 Remsen street, Brooklyn, N. Y.; John E. Weeks, M. D., 43 West 18th street, New York; Richmond Lennox, M. D., 164 Montague street, Brooklyn, N. Y.

"One morn, a Perie, at the gate
of Eden stood, disconsolate."